This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

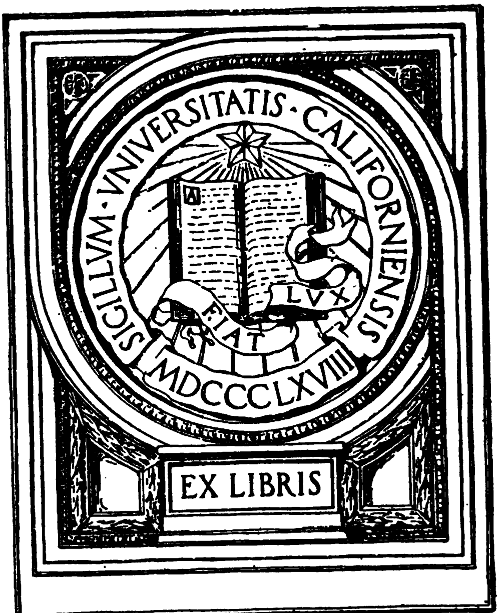
+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`


SIGILLVM · VNIVERSITATIS · CALIFORNIENSIS
FIAT LVX
MDCCCLXVIII

EX LIBRIS

A SYSTEM

OF

GEOLOGY,

WITH

A THEORY OF THE EARTH,

AND AN EXPLANATION OF ITS CONNEXION,

WITH THE SACRED RECORDS.

BY

JOHN MACCULLOCH, M. D., F. R. S., &c.

IN TWO VOLUMES.

VOL. I.

LONDON:
LONGMAN, REES, ORME, BROWN, AND GREEN,
PATERNOSTER ROW.

1831.

QE26
M3
v.1

EARTH
SCIENCES
LIBRARY

In Memoriam
Arthur Eaton

LONDON:
Printed by J. Teuten, 14, Dean Street, Soho.

690697

CONTENTS.

Chap. Page.

I. *On the general Objects of Geological Science* 1

II. *On the particular Objects and Ends of Geological Investigation* 8

III. *On the General Form and Constitution of the Globe* 16

IV. *On the general Disposition of the Surface of the Globe* 31

V. *On the general Outlines, or the picturesque Characters of Rocks* 52

VI. *On the general Distribution of the Materials which constitute the Earth* . . . 60

VII. *On the Characters and Forms of Strata, and on Stratification* 67

VIII. *On the Disposition, Fractures, and Dislocations of Strata* 86

IX. *On the Flexures and Contortions of Rocks* . 111

X. *On the Characters and Disposition of unstratified Rocks and Veins* 131

XI. *On the concretionary and crystalline Structures of Rocks* 166

XII. *On the Origin, Materials, Composition, and Analogies of Rocks* 192

XIII. *On the Destruction of Rocks* . . . 234

XIV. *On the particular order of succession among Rocks* 268

XV. *On the elevated [illegible] Alluvia* . . 298

690897

Chap.		Page.
XVI.	*On the Deposits called tertiary and fresh-water Formations*	313
XVII.	*On the Formation of Coral Islands* . .	334
XVIII.	*On Volcanoes and Earthquakes* . .	349
XIX.	*On Mineral Veins*	379
XX.	*On the geological Relations, of the organic Fossils*	406
XXI.	*On the successive Forms of the Earth: Revolutions of the Globe*	455

ERRATA.

The collocation of some chapters having been changed during the printing of the second volume, the reader must refer to the table of contents for the purpose of correcting those numbers.

Page.	Line.				
27	— 20	*for*	statistical	*read*	statical
76	— 17	*for*	about	*read*	above
77	— 8	*after*	ceases *insert a comma*		
174	— 26	*for*	arrangements	*read*	arrangement
304	— 31	*after*	wanting *insert a comma*		
485	— 20	*for*	sunk	*read*	sank
493	— 9	*for*	strata	*read*	rocks

PREFACE.

THIS WORK was written in 1821, and it has therefore slept even beyond the Horatian period. I have waited ten years, in the announced hope that some better man would stand forward in the breach, the representative of Geological science as it now is: endeavouring also to tempt others to this perilous honour, by the occasional publication of certain portions. I do not see that it has been superseded; nor that there are any hopes from a longer delay. I did also expect that time would have brought material improvements, through the efforts of the present multitude of labourers. I grieve to say that it has scarcely received a valuable addition from this source, and not a single fundamental one. The evidences of Geology have indeed been multiplied; yet through identical facts only: since I do not perceive that a new one has been added to the Science. This ought not to have been.

Having already published every thing, derived from my own observation, on which the present attempt is built, these have long been as much the common property as what I have borrowed. Whether those to whom I have so long left this task, have thought that it could not be executed, or should not be attempted, or whether there be any other reason, it is not for me to conjecture. But he who says that System is a presumptuous title, does not know the meaning of that word; since it neither implies perfection, nor the pretence to it. It is a view of the actual condition of any Science at the time; and all have had their tentative systems or approximations. Every science now possesses one: yet not even Astronomy is perfect.

Systems approach perfection by degrees: and he who produces one, facilitates the production of a better. He also who makes the first attempt is not to be envied: since all are as ready to censure its defects as they are able to see what the author himself points out: choosing to forget that he has done so, while ignorant or neglectful of what has been done and what overcome. The traveller over a solid road knows nothing of the toil that first laid it on the quaking morass, and as little does he care; while he is enraged at the casual rut. But though such a System should teach nothing, it would be valuable by the display of its very defects. No chasm can be filled till it is known: while thus are errors and blanks classed with the nearest truths, so as to indicate the way by which they are to be corrected and supplied. All sciences commence with error; and he who waits for the hour of perfection, suppressing what he knows, because too proud or too timid to confess a pardonable ignorance, is the Σχολαστικος who will not enter the water till he can swim. Nor will the day of truth ever arrive, if they who can add to the stock of knowledge confine it within themselves.

Be the result what it may, it is my duty to say, that it rests, with some exceptions, on my own observations, confirming those of others, when not original. Observers may differ; but Nature is always the same. And it was my duty to confirm my references to authorities, by applying to the great source of all Authority, wherever this was possible. Thence have I quoted, more in confirmation than for any other purpose: never trusting, even to names of reputed weight, when the facts were contradictory to those which I had verified, or to just analogies, or to the principles of the collateral sciences; assured that Nature cannot differ from herself, nor one truth oppose another. These are the real grounds on which the validity of authorities must be tried: and hence perhaps have I quoted fewer than might have been expected; yet, I think, enough; since an indiscriminate or careless reliance on them is a most perilous foundation for a work on this Science. An ambitious multiplicity of writers, sectarianism, and hypothesis, must ever be the source of bad observations, if not also of gratuitous assertions: while an established phraseology gilds all this with the aspect of knowledge. No system of geology can yet be compiled from the works of others; though often attempted. Kirwan has revelled in authorities to prove how much we knew: a later writer has proved, from the same authorities, that we know nothing. Nor does any one who even aspires to the name of philosopher, suffer another to reason for him; as, never yet was any system in philosophy founded on the eyes or the opinions of others.

Geologists have been acused of founding theories upon single and favoured districts; yet have I drawn my chief illustrations from Britain. It is true: but there is no resemblance in the applications: as I can also justify this proceeding. Geological facts have no relation to geography: the earth is every where of the same general structure. And I need not hesitate to say, that excepting volcanoes, and little more, this little island contains every fact in the world, with much that is almost peculiar to itself; and that more knowledge can be acquired from a careful examination of it, than from all the writings of all those who have prided themselves on the extent of their travels. The study of Arran alone has taught us more than Asia and America united; but there is an assumption of grandeur in quoting these. Yet there is a better reason: for thus can British readers verify the asserted truths, and thus also turn from the writer to his Teacher; to that Nature where he also will see and learn for himself.

If, to controvert, be termed controversy, I am sorry for what I could not avoid. I neither envy the taste nor the feelings that delight in this bane of modern science and literature: under which, they who can contribute nothing, seek for fame, by depretiating what they even rarely understand. Never has there been a science, unless it be physic, so encumbered with rubbish as geology: it was impossible to move a single step without clearing it away. He who desired to built on the solid rock of Nature could not but attempt to remove the ruins that obscured it. And whoever seeks to " make his understanding a repository of truth for his own sake, rather than the warehouse of other mens false and inconclusive reasonings," will follow the same plan.

Of the arrangement of this work I have little to say. I have referred to my Classification of Rocks as the grammar of this science, while avoiding, as much as possible, all collision between them. If the order of the subjects does not prove satisfactory, I will gladly hear of a better; yet it must be from one who has bestowed equal thought on an unexceptionable arrangement. If there are repetitions, and if references to things not yet examined, I shall be pleased to see any plan that will remove this blot from one place, without leaving an equal or worse one somewhere else.

The work is too long, because it includes essays on several subjects. I would gladly know where else the needful facts and reasonings are to be found: and should be still better pleased if geological science were in such a condition, that a system might consist of a series of enunciations or axioms. It is not yet time for magisterial language: and the decisions of former system makers have already sufficiently obstructed the progress of geology. Without details of facts, it would have been useless to the student: and, without reasoning these into evidence, it would have merited the fate of the German cosmogonies. In my own opinion, it is far too short: and it is the learner who shall judge between us. I am sensible that the sketch of a Theory of the Earth requires a volume, instead of a Chapter; and further, that it can scarcely be understood without that series of drawings, as a guide through a perfect labyrinth of reflections, without which I could not even have written it. It must remain for others to demand such a volume: and if it is not less true that illustrations would have been most useful to the whole work, as they had been prepared for it, there are few who do not know where the obstruction lies, as it is they alone who can remove it.

That this work abounds in defects, I know better than most of my readers: but it is no very heavy crime to have failed in producing a complete one from deficient elements. As to errors and omissions, there is no fear that they will not be noted; and long before they will be corrected and supplied. Of mere hypothesis, I hope there is none: while perfectly indifferent whence Truth comes, provided it comes at all. And thus may I safely say with Cicero. "Nosque ipsos redargui refellique patiamur; quod ii ferunt animo iniquo qui certis quibusdam destinatisque sententiis quasi addicti et consecrati sunt, eaque necessitate constricti, ut etiam quæ non probare soleant, ea cogantur constantiæ causa defendere: nos qui sequimur probabilia, nec ultra quam id quod verisimile occurrit progredi possumus, et refellere sine pertinacia et refelli sine iracundia parati sumus."

UNIV. OF CALIFORNIA

GEOLOGY.

CHAP. I.

On the general Objects of Geological Science.

THE slightest examination of the surface of the Earth displays to us numerous irregularities by which its spherical form is modified; and, when we penetrate within it, we discover that it contains many rocks of different characters, disposed in a confused manner, and giving rise to the inequalities which form its mountains and its valleys, which are the immediate causes of the elevation of its continents and the depression of its seas. The study and description of these substances and these appearances, constitute the natural history of the Earth, according to the simplest view of that science. Thus this branch of Natural History comprises those circumstances in the disposition of the surface, which do not appertain to geography; together with the distribution, the mutual relations, and the nature, of all the substances which enter into its composition. These again subdivide themselves into many inferior branches; including the history of minerals, of organic fossils, and of alluvial deposits; with other matters which it is not here necessary to specify.

These objects are not merely adapted to the gratification of speculative curiosity, but are, in many cases, conducive to the wants or luxuries of mankind, to innumerable uses in the arts of life. Thus it becomes necessary to form accurate records of their places, by means of mineral topography, to indicate the precise circumstances under which they exist, the modes by which they may be best obtained, and the varieties of character which they present: the whole constituting a body of investigations by which geology is raised to the rank of a practical science.

If now, as in the other branches of natural history, every rock and substance were visible, or could be exhibited by the mere efforts of industry, or if the geological surface of the earth were, like its geographical details, within the reach of actual measurement and examination, time and industry alone would procure for us all the information which could be required, whether for the purposes of curiosity or use. A perfect accumulation of facts would, as in botany, shortly form the materials of a science, to which the philosopher would superadd those analogies or distinctions by which nature has classified or separated all its objects.

But such is the condition of the earth's surface, and such its structure, that we can make little progress in its study without in some degree inverting this process. As in geometrical synthesis or in chemical science, we must extend the conclusions and results obtained from the observation of a certain number of facts, to the purpose of discovering others; and thus the geologist is destined to pursue his investigations and extend his discoveries, by the consideration of analogies, and by inferences from limited observations; by a combination of the operations of analysis and synthesis, according to the rigid rules of philosophical induction. Geology thus necessarily assumes a high rank among the sciences; and if it has not yet obtained to such an eminence as to claim a place among those which are esteemed accurate, its cultivators have the greater stimulus to observe rightly and reason truly; that they may place it on that proud elevation in the honours of which they must themselves participate.

To illustrate these remarks by a reference to the actual state of geological knowledge, to the facts and analogies already ascertained, to those by which a mere observer may be misled, and to those modes of proceeding by which the truth may be established, would be abundantly easy. But, to the student, it would now require the anticipation of many details; and hereafter, when he shall have been put into possession of them, it will no longer be necessary.

Disgusted with visionary theories, actuated by a spirit of opposition, or influenced by narrow views, many geologists have wished to stop short in this career; surrounding themselves with a circumscribed boundary, and refusing to inquire into those revolutions, of which the earth every where presents the most impressive traces, to attempt an explanation of their causes, or to connect, by a just theory, all those marks of change which are the proper objects of a philosophical system of Geology. Others, have opposed the progress of rational geology, by confounding primary and secondary causes. Because the poetical imagination of Buffon has created worlds, they refuse to inquire into the agencies by which the materials of this earth have been disposed and modified, by which they are alternately renewed and destroyed, by which all the changes, of which it displays the marks, are caused to work together to one great end. But even the philosophical geologist does not inquire how the great Creator of the universe produced the globe that we inhabit. He is content with investigating the secondary causes by which its materials assumed their present form and disposition, the laws which regulate those incessant changes by which every thing is alternately destroyed and renewed, yet where nothing of all that has been created is lost. Had astronomers been content to know that the earth was a sphere and that the planets performed their revolutions in stated times, had the force of gravity remained unknown because they refused to investigate secondary causes, the nature of the tides would yet have been a mystery, like the complicated motions of the moon, and comets would still have been objects of astonishment and terror.

But the human mind is so constituted that it cannot rest content with facts. If it possesses innate propensities, the investigation of causes is assuredly one of them. The very geologist who disclaims all theory, has his own; the lowest of the vulgar desire reasons. We cannot open our eyes without seeing daily changes on the surface of our globe. Rivers alter their courses, and lakes are obliterated, by the transportation of earth. Mountains are levelled with the plains by the action of rains and frost; and valleys are filled up by their ruins. An earthquake alters, in an instant, the whole face of a country, and a volcano overwhelms it with new rocks. These phenomena irresistibly compel us to inquire whether similar causes may not, in distant times, have produced analogous effects; whether they are capable of explaining the appearances which meet our view. Hence the investigation of secondary causes becomes inevitable; and hence geological theories become inseparable from geological appearances. The laws which govern the phenomena of nature, force themselves irresistibly on our attention. They are strictly involved with the analogies which regulate all our reasonings and direct our observations; and, without them, we cannot proceed a step on firm ground. They distinguish the philosopher from the empiric; and combine scattered observations into a body of useful and rational science. Even in the science of Nature, as in that of numbers, the assumption of imaginary or erroneous laws, leads to the discovery of the true. The history of astronomy is, in itself, a lesson to those who ignorantly undervalue the pursuit of general laws. Bewildered in spheres and vortices, it arose, as in a moment, complete, from the theory of gravitation.

Hence the consideration of secondary causes forms, not only a legitimate, but an essential part of geological science. That science, like all others, comprises the history of all the facts which it involves; and, from these, it establishes certain general analogies. Ascending a step higher, it declares the laws which have regulated, and will continue to regulate, all the phenomena of the globe; and thus finally establishes a legitimate theory of the Earth.

But a work of this extent, involving such an extensive series of phenomena, cannot soon be perfected. It may not yet have been even sketched; since, of the truth of those schemes which have been proposed, we can have no valid evidence but that arising from their adaptation to a majority of those phenomena, of which not many have yet been examined with sufficient care. Many theories may be reconcileable with a superficial view of geological phenomena; but the formation of a correct one requires an accurate and an extensive acquaintance with these. Few researches are more complicated and arduous; and there are none, of which the facts are more diversified, more dispersed, and more difficult to disentangle. Nor are there any, of which the causes are more difficult of access, and further removed from the common track of observation and experiment. Yet the general improvements of science, and the light which all its branches mutually throw on each other, cannot probably fail, ultimately, to place geological science on a firm and durable foundation. Its very difficulties present the highest inducements which can be offered to an ambitious mind; and he will have little reason to congratulate himself on the results of his industry or acuteness, who, disclaiming the true ends of philosophy, is content with the objects that occupy his cabinet, with the discovery or the classification of a rock.

He whose views are formed in a right spirit, will not rest here. Science requires of him much more. He must be familiar with the wider and greater relations of all the substances in nature; his duty is to combine a careful study of particular instances with comprehensive general views, to compare the minutest analogies, to contrast the most insensible differences, and to explain obscure or doubtful appearances by others of a clear and decided character.

He will pursue nature amidst her minutest as among her most gigantic features; exploring her in every variety and gradation of her works, and always remembering that she can only be understood by him whose mind can alike apprehend the parts and grasp the whole. But it is his duty also to recollect, that, by premature generalization, he may obstruct his own progress and that of his Science. To fill a blank is tempting: but a vacuity is often preferable to a blank badly filled. The imperfection is less sensible than the defect; while it is apt so to establish itself as to be with difficulty discarded.

CHAP. II.

On the particular Objects and Ends of Geological Investigation.

A VERY superficial examination of the objects which surround us, is sufficient to show that the substances which constitute the accessible portions of the globe, are numerous and various, and that their positions and mutual relations are irregular and intricate. Granite and marble, slate and sandstone, furnish the materials of architecture, the metals and coal are the foundation of the arts. The same rocks that constitute the ridge of Jura occupy the plains of England, and the basalts that repose on the granites of the Andes are found beneath the limestones of Sky. A thousand phenomena record the revolutions which these substances have undergone. The fragments of former rocks are reconsolidated to form new mountains, the remains of animals that have existed beneath the ocean are imbedded in the loftiest Alps, and the vegetables that once flourished in the light of day are buried beneath the solid strata.

A very general view of the objects which comprise the immediate pursuit of the geologist, will here be sufficient. The most obvious of these is the nature of the rocks which constitute the accessible portions of the globe; and, in these, it is necessary to distinguish the mineral composition, the peculiarities of structure, the varieties which they may present, and the families or species into which they have been already divided, or by which they may be arranged. Their peculiarities of form and disposition, whether constituting irregular masses, strata, or veins, are subsequent objects of consideration; and their natural history is rendered complete by investigating their order of succession or their other mutual relations, the influence which they may exert on each other, the general analogies which they bear to the whole system, and the causes, nature, and consequences of the changes which they have undergone or may be undergoing. Lastly, for the purposes of mineral topography, it is necessary to determine their geographical boundaries: the geologist being thus enabled, by the aid of maps and sections, to refer accurately to them, whether for œconomical objects or the mere purpose of elucidation. The examination of mineral or metallic veins, forms another distinct object of geological investigation: nor is the geologist exempted from the study of minerals, though mineralogy has been erected into a separate pursuit. The mineralogist may pursue the minutiæ of his own department, with little aid from geology; but he will be a very imperfect geologist who is not acquainted with those objects, for the discovery of which, mineralogy will most frequently be indebted to him.

If the multitude and variety of organic remains shall appear sufficient to exempt the geologist from a minute investigation of all these objects, and to permit him to divide this labour with the cultivators of Zoology and Botany, still it is his especial duty to determine the substances in which they are imbedded, the nature and relative antiquity of these, and a multitude of other circumstances which it would now be superfluous to detail. For his own immediate ends, he must possess, at least a considerable knowledge of the characters and analogies of fossil animals and vegetables, and of the relations which they bear to the corresponding beings which now inhabit the earth. But he must not forget, at the same time, that Extinct Zoology and Botany do not constitute Geology; and, in the study of the inhabitants of our earth, forget the earth itself. The general bearings of these belong to this science: but the objects belong to other departments of nature.

An examination of the changes actually taking place on the surface of the earth, and of those which have formerly occurred, constitute another distinct branch of the pursuits of a geologist. These include the waste and degradation of mountains, and the consequent results; together with the various causes by which their materials have been thus transferred.

The phenomena of volcanoes form the last division of those pursuits which it is necessary to enumerate. In these, whether living or extinct, his attention is called to the appearances which their eruptions display, and the consequences resulting from them; to the rocks which they have formed, and to the nature of those which they have overwhelmed. Lastly, amid all these facts, among these various objects and actions, he must seek for those analogies, and assign those causes, on which all the appearances before him depend.

On such a basis may a scheme of geological investigation be conceived; but we are not yet in a condition to form a regular one.

Were all the facts and analogies that appertain to Geology attained and proved, it would be easy to treat the subject and its proofs in a systematic order. But it would then be the science which we are as yet but seeking. In this defect, however, it only partakes with all the Natural sciences: whether exceeding them, or otherwise, in that respect, I need not here inquire.

The present duty of a systematical inquiry, is to describe objects and actions which cannot all yet be classed under general divisions and laws; and it cannot avoid proposing analogies and inferences which it is unable to prove; while it must also make use of proofs which are themselves to be proved hereafter, or prove several laws by one or more facts, or the reverse; thus necessarily becoming entangled among future and retrospective references. Hence, the order which would be desired, cannot be adopted. And hence also it may be useful to give a sketch of an order; which, however dry and repulsive it may appear, will serve to indicate the catenation of the facts and inferences.

The materials of this inquiry are Objects and Actions; the result constitutes Inferences; and these are retrospective, as well as present and future. The Retrospect is the material for a Theory of the Earth.

OBJECTS.

The Objects are the MATERIALS of the Earth.

The Materials are, ROCKS and FRAGMENTS.

The Rocks are, STRATIFIED and UNSTRATIFIED.

ROCKS and FRAGMENTS are formed of the same substances.

The substances are simple earths, including oxydes, or minerals.

Rocks, and the larger Fragments, are composed of Earths or of Minerals, and of Animal and Vegetable matters compacted.

They are compacted by Mechanical approximation, or by Chemical action, or by both united.

ROCKS.

STRATIFIED ROCKS have been deposited from water.

They have been produced from fragments, or from dissolved substances, or from both.

They have been consolidated by mechanical forces, or by chemical actions, or by both.

They were once horizontal in position, or nearly so, and their positions are now various.

They were once continuous and straight planes, or nearly so, as far as their extent; and they are now bent, fractured, and separated.

They were once unmixed with unstratified rocks, and they are now intermixed with these.

Being essentially composed of minerals, they also contain the remains of animals and vegetables.

They were once, or oftener, below water, and they are now above it.

They are repeated in consecutive and parallel order, of the same, or of different kinds.

Every consecutive and parallel series was formed beneath one water, and during one defined period of repose.

There are more than one or two sets of the consecutive and parallel, separated from the approximate, which occupy other and different angles.

There is a frequent, if not a necessary, interposition of consolidated fragments at these points.

With rare exceptions, every stratum is of later origin than the one next below it.

UNSTRATIFIED ROCKS have been produced from below the stratified.

They are found below these, or above them, or intermixed in the forms of masses, beds, and veins.

The intermixture is attended by mechanical and chemical changes in the stratified rocks.

They have been consolidated after fusion, and their structure is necessarily chemical.

Volcanic rocks present all the same characters.

An unstratified or a volcanic rock may be of later origin than the stratum above it.

FRAGMENTS.

These consist of portions of rocks, of minerals, of earths, and of vegetable and animal substances.

They occupy the surface of the solid earth, either above the water or beneath it.

They have been produced by the destruction of rocks, and by vegetables and animals.

They have been deposited by gravity and by the motion of water.

ACTIONS.

Actions are the results of animal and vegetable life and destruction, of water and the force of gravity, or of fire.

By organic production and destruction, its objects become portions of the Fragments, or form Strata, or parts of these.

By Water and Gravity, the solid rocks are broken into fragments, and deposited on the land or beneath the water

By Water, animal remains are mineralized, and vegetable ones bituminized; both classes of change being of graduating characters.

Fire acts in Volcanoes, which are visible or invisible.

It elevates the superincumbent materials of the earth, whether solid or otherwise.

It produces fluids which become rocks; and these are formed beneath, or among the rocks of the earth, or above them.

These rocks possess the various characters of the Unstratified ones, and produce the same effects.

Among the consequences of elevation, are the production of displacements in the superincumbent water, and of torrents or currents.

RETROSPECT.

The inferences from Objects and Actions connect the present with the Past; and, in a more limited manner, with the Future.

The fragments, and the solution, of former rocks, and earths, in former Water, produced the present Stratified Rocks.

The effects of former Fire, produced the Unstratified Rocks, with the consequences attributed to them.

Former races of living animals and vegetables, in different waters and on different lands, produced the objects of this nature now found in rocks and fragments.

The successive consecutions of distinct parallelisms among the Stratified rocks, infer as many distinct conditions of the globe.

The time requisite for the production of Stratified Rocks and for the reproduction of Animals and Vegetables, imply prolonged intervals between each condition.

The powers which produced the unstratified rocks and displaced the stratified, were the immediate causes of the revolutions.

Each change from one Condition to another, implies one Revolution.

The predominant Revolutions have consisted in the elevation of the submarine stratified rocks; but, in one case, possibly in more, the revolution has been a depression of rocks beneath the water.

The interior of the Earth contains a permanent source of heat, competent to the powers of fusion and expansion.

A certain portion of it is permanently fluid.

It contains, or has contained, cavities.

It consists of matter heavier than any known rocks.

It approximates to the figure which would be produced by its diurnal revolution were it a fluid.

With respect to the FUTURE, it is inferred that the present actions are tending to produce a new condition, analogous to that which is just past.

The duty and purpose of the imperfect investigation, the only one yet in our power, is to examine the classes of Objects and Actions, to indicate the inferences, and, lastly, to describe the remaining Objects which belong to Geology, as a branch of Natural History; or as an Art, of which some comprise or repeat necessary proofs, others add superfluous ones, and a third set are as yet incapable of classification for the purposes of the Science.

CHAP. III.

On the general Form and Constitution of the Globe.

THERE are many points in the history of our globe, respecting which Geology and Astronomy mutually throw light on each other. If the variations of gravity on different parts of the surface, and the peculiarities of figure on which they in some measure depend, are subjects for the especial consideration of the Astronomer, it is the duty of Geology to investigate those circumstances in the history and condition of the earth with which they are connected.

Although, in a popular and general sense, the form of the earth is that of a globe, it has long since been established, by the measurement of degrees on different parts of its surface, that its figure is not spherical. These trials having shown that the meridional degrees increased in length from the equator towards the poles, it followed that the radius of curvature was less, or shorter, near the former than the latter, or that the earth formed an oblate spheroid, of which the polar axis was less than the equatorial.

Thus, that which had been previously suspected from mathematical considerations, became apparently proved by mathematical experiments; namely, that the form of the earth tended, at least, to that which would result from its fluidity at some period of its existence, combined with the rotatory motion of all its parts on the polar axis.

For this important fact, geology is indebted to astronomy, which thus establishes, if not an original or subsequent state of universal fluidity in the earth, a succession of actions, at least, in which a certain portion of it has either been fluid at one period, or at successive periods, or has, in some other way, been so possessed of internal mobility as to have been capable of fulfilling the conditions of this problem.

Numerous experiments and observations, followed by the requisite calculations, and from different data, have been made, at different times, for the purpose of determining the exact figure of this spheroid; and the subject has also recently been resumed with considerable ardour and anxiety. It would here be to transgress the proper bounds of this sketch, to detail the whole of this subject, which is of considerable extent; nor need I even give the whole of what had been concluded as to it by different mathematicians, since the more recent calculations will be sufficient for the purpose here in view. Though it has resulted, however, that the form of this ellipsoid is not that which, from abstract mathematical considerations, it had been conceived, we are scarcely yet entitled to suppose that even the most recent conclusions have truly solved this problem. But I shall merely tabulate these results without further commentary or explanation, as they would be unsuitable in this place. The ellipticities are given in parts of the equatorial axis, as usual, and require no other explanation.

1.	Sir Isaac Newton - - - -	$\frac{1}{230}$
2.	Playfair, from the Meridians of Peru, and between Dunkirk and Perpignan -	$\frac{1}{300}$
3.	Ditto, from the Meridian in Peru and that between Clifton and Dunnose -	$\frac{1}{332,68}$
4.	French report in the Système Métrique	$\frac{1}{386}$

5. La Place, from fifteen pendulums -	$\frac{1}{319}$
6. Ditto, a second, from more arcs -	$\frac{1}{308}$
7. Ditto, by analysis from pendulums, and from the lunar inequalities arising from its oblateness - - -	$\frac{1}{306,57}$
8. Ditto, a second, from further similar observations - - -	$\frac{1}{310}$
9. Mr. Ivory - - -	$\frac{1}{296,5}$
10. Sabine, from pendulums, by Clairault's formula - - -	$\frac{1}{288,4}$
11. Ditto, from Kater's observations -	$\frac{1}{289,5}$
12. Ditto, from Arago and Biot's - -	$\frac{1}{288,5}$
13. Medium - - -	$\frac{1}{289,1}$

Such is the present state of this question; but it has also been conceived, from different observations, that the meridians are not themselves elliptical; and the law of the diminution has further appeared, to some, to be unexpectedly irregular, as if the figure of the earth was very complicated; while the examination of measurements made to the south of the equator, at the Cape of Good Hope, and elsewhere, seems further to prove that, so far from being a solid of revolution, the two hemispheres on the opposite sides of the equator do not correspond, either in figure or magnitude. But I must pass from this subject, as yet far too obscure to permit us to feel satisfied respecting many of these supposed results.

If the earth were a homogeneous fluid, with the force of gravity placed in its centre, and diminishing in the other parts of the mass in the direct ratios of their distances from that point, the spheroid must be an ellipsoid of revolution; as no other form will produce the equilibrium of all the imaginary columns between the centre and circumference, which, under the circumstances of a greater centrifugal force in the equatorial parts, and the consequent diminution of gravity in these regions, must balance each other.

Now as, on the surface of the earth, there is a diminution of gravity as we proceed from the poles to the equator, depending partly on the greater distance of the equatorial surface from the centre, and partly on the greater quantity of its centrifugal motion, we can determine what this force ought to be at different points; and if the Newtonian hypothesis be admitted, the force of gravity at the poles will exceed that at the equator by a similar quantity.

But the actual force of gravity on the different parts of the surface may be measured by the pendulum, as the tabular view just given indicates: a greater number of vibrations being performed in a given time, in proportion as that force increases at any particular point. Thus astronomy furnishes both calculations and experiments from which geology may derive information relating to the interior parts of the earth, and may learn whether the whole globe is of homogeneous density or not. For if we assume the Newtonian hypothesis, the force of gravity should increase from the equator to the pole, so that the latter should exceed the former by $\frac{1}{230}$ part; or if the force of gravity at the equator be expressed by 1, its increase at the pole should be 0,00435. But observations on the oscillations of the pendulum in different latitudes gave reason to suppose that this increase is 0,00567; and hence it would be inferred that the earth is not homogeneous. It has been truly said, in observing on this conclusion, that the earth needs not therefore be stratified throughout: nor is there indeed any geologist who now believes this: but when it is said that the increase of density towards the centre does not prevent it from being a homogeneous substance, chemically speaking, it is to conjecture of what chemistry is unable to comprehend at present, from not knowing what the consequences of extreme compression would be on any substance.

The same general results are deducible from those experiments by which the actual specific gravity of the globe of the earth has been determined: it ought rather to be said, by which an approximation has been made to the solution of this problem. It is not here necessary to describe the experiment of Mr. Cavendish; but a brief sketch of that of Dr. Maskelyne, will not be uninteresting to the geological reader; as its accuracy involves certain questions respecting the nature and disposition of the rocks at the surface of the earth.

In this experiment, a trigonometrical survey of the mountain Schihallien was made, by which its figure and bulk, as far as these were necessary, were ascertained; Meridional stations were then selected on opposite sides, and the distance between these being referred to the known length of the meridian at that parallel, their latitudes were inferred The apparent difference of latitude, deduced from the zenith distances of certain stars observed from the two stations, being then compared with that obtained from the trigonometrical measurement, was found to be 11″,6 greater. Thus the sum of the deviations of the plumb line at the north and south observatories was computed; the zenith positions having been separated from each other in consequence of the attractions of the plummet towards the vertical, by the mass of the hill. By subsequently comparing the attraction of this mass with that of the whole earth, or to the force of gravity, it was found to be as 1 to 17804.

The mean density of the earth to that of the mountain was afterwards found to be nearly as 9 to 5, or more precisely as 17804 to 9933. Thus it was concluded that it must be composed of substances such, that their mean density must be nearly the double of those rocks which compose Schihallien. For this purpose, it was necessary to compare the weight of the matter composing the mass of this hill with that of water, or to ascertain its Specific gravity; an element required for determining the mean Specific gravity of the terrestrial globe, which was sought.

It was at first assumed that the Specific gravity of Schihallien was 3; geological knowledge not having at that time made much progress in this country. Geologists will be pleased in reflecting, that as the labours of mathematicians and astronomers have contributed to throw light on some of the most obscure parts of their science, so, in return, has geology furnished the means of completing the data for an astronomical problem, which, without its assistance, must have ever remained imperfect, and produced erroneous results. The mineralogical investigations of Mr. Playfair reduced that specific gravity to 2,7 or 2,8; and if 2,75 therefore be assumed as the specific gravity of Schihallien, it will prove that the mean specific gravity of the earth, obtained in this way, is about 5.

This determination, however, can only be considered as an approximation to the truth. It interests geologists, even more than mathematicians, to consider why it is not perfect; since the doubtful element of the calculation rests with their science. It is necessary that the actual weight, or specific gravity, of the spheroid into which the attracting mass of Schihallien is resolved, should be known. Without detracting from the merits of Mr. Playfair, it is more than doubtful whether he has succeeded in determining this point. If the form and situation of this mountain were such as to offer great conveniences to the mathematicians engaged in this problem, it is, unfortunately, deficient in uniformity and simplicity of structure. Not only is it a matter of extreme difficulty to ascertain the true distribution of the strata throughout the whole mass, but the specific gravities of the different materials vary in such a manner as to vacillate between 2,4 and 3, or even more. All the strata are, at the same time, elevated at high, but unequal angles; while it is also difficult to discover their relative proportions on the surface, and, still more, the positions which the rocks of different specific gravities assume in the interior of the mountain; a circumstance of considerable importance, on account of the angular differences of the action of the several columns on the plummet. The different strata, it must be remarked, consist of quartz rock, micaceous schist, hornblende schist, and limestone.

Under these uncertainties, a true mean specific gravity could neither be determined nor applied; that element could only have been perfect, in this case, when the actual specific gravity of each attracting part of the hill, and the quantity of each, were ascertained. A tolerable approximation, it is probable, has nevertheless been made.

It must now however be further observed, that such are the difficulties which attend an accurate solution of this important problem by this method, that the plan pursued by Mr. Cavendish is preferred by mathematicians, as are the results which have been obtained by means of it. La Place, nearly following this, makes the mean density of the Earth to be 5½ instead of 5, as Cavendish had made it 5,48, and allows 2¼ for the superficial. It has been said that this latter computation is too low, as there are no rocks which are so low in specific gracity as 2¼; and it has been proposed to make it 2¾, as Playfair has done; so as to allow 5,48, as Mr. Cavendish had done before, for the mean density, or, taking a medium, 5,4. But it must be remembered that the density of the ocean, which is little more than 1, is to be taken largely into the estimate for the superficial parts; so that La Place's computation is perhaps, after all, the nearest to the truth.

It now follows, that the matter which forms the earth increases in density from the circumference towards the centre. The law deduced from the experiments on the specific gravity of the globe, coincides with the results which have already been shown to follow from comparing the ellipsoid of the earth on the principle of its homogeneity, with observations on the vibrations of the pendulum. That ellipsoid is less unequal than it should be were it homogeneous throughout.

The irregularity of the terrestrial spheroid has been already argued from the measurement of degrees on different parts of its surface: it will not be useless to show how this conclusion is thought to be confirmed by observations on the variations in the force of the pendulum in different places.

If the earth be supposed an ellipsoid, the force of gravity at its different points may be determined by calculation; and hence the pendulum may be applied to verify this regularity, just as it was shown capable of ascertaining whether the matter of the earth was homogeneous or not. The figure of the earth may consequently be determined, either by a direct measurement of the degrees on different parts of the surface, or by observations on the length of a pendulum required to produce, in the same places, a determined number of vibrations in a given time. On making these comparisons, the coincidence of the results is found to be so imperfect, as to have led also to the conclusion that the figure of the earth is far more complicated than might have been imagined; and it has thus been thought to present notable irregularities. That many of the discordant results in question are the consequence of real and important irregularities in the figure of the terrestrial spheroid, may prove to be true; but many at least may, with equal probability, be sought for in the dispositions, magnitude, and elevation of continents, and islands, and in the varying depth of the sea; while others are probably the consequence of errors caused by the varying density of substances which lie at the surface, or constitute the deeper seated parts of the globe.

The consequences more particularly interesting to geology which follow from the preceding observations, relate principally to the matter and the composition of the earth, and to the events which have been the causes of its actual form. Supposing the elliptic figure to be demonstrated, and the corresponding increase of lengths in isochronous pendulums, from the equator to the pole to show a regular increase of gravitation in the same manner, the earth should be symmetrical, or the densities round the centre equal, and regularly increasing inwards.

As it is not necessary for the present purpose to be very minute, it is sufficient to say that the heaviest rock with which we are acquainted at the surface of the earth is about 3, and the lightest little more than 2¼; while the specific gravity of the ocean, which forms so large a part of the surface scarcely exceeds 1. To compute the mean density of the superficial parts of the solid earth, is evidently impossible; but it probably cannot much exceed 2¼, as already observed; and as the depth of the ocean has been proved by La Place to be necessarily considerable, while we are equally sure that we have deep access to the earth, from the elevation of mountainous strata, we may safely suppose a superficial crust, of many miles in depth, including the water and the land, not exceeding 2¼ in weight. But the mean density of the earth itself is 5½ or more than double that quantity; and hence the interior parts of the globe must possess a greater specific gravity than 5½, to counterbalance this want of weight at the surface. It is also clear, that this specific gravity must be even such as to be equivalent to that of many metals; and hence it has been conjectured that, instead of being formed of rock, the interior parts of the earth must be, in some degree at least, of a metallic nature. If, as was also shown from mathematical considerations, there is a gradual increase of density towards the centre, it becomes still more probable, that the nucleus of the earth is metallic. This supposition has been supposed to receive additional force from the phenomena of magnetism; the nature and position of the magnetic centres having been imagined to imply the existence of a central mass of iron: and it is thought to be confirmed still further by those chemical facts which teach us that the rocks are formed of metallic oxydes.

But what the density of the centre of the globe ought to be, on this view of its structure, has also been computed on the theory of compressibility. If 5,4 be taken as the mean density, instead of 5,5 as given by La Place, and the ellipticities be taken respectively at $\frac{1}{285}$, $\frac{1}{294}$, $\frac{1}{305}$, and $\frac{1}{312}$, the superficial densities being assumed as 3.13, 3.2, 2.79, and 2.60; the densities at the centre will be 13.25, 14.54, 15.78, and 20.10 respectively. The least of these, it is apparent, is nearly double that of iron, and the greatest exceeds that of gold. Hence we must remain utterly at a loss respecting the true nature of its interior parts; nor is there aught to be gained by conjectures on this unattainable subject.

It remains to consider in what manner the earth has assumed that form which, if it is not actually a spheroid of revolution, approaches to it in a general manner; being modified by greater or less irregularities. These irregularities are peculiarly interesting to geology; since they must be considered as depending on other causes than those from which the earth would have assumed the true shape of the ellipsoid of revolution. No theory of the earth can be perfect which does not provide for both: but geology can only offer some detached reasonings on the subject, unsupported as it is by a sufficient foundation of facts.

The subject naturally divides itself into two parts; namely, a consideration of those supposed irregularities which cause the general form of the earth to deviate from the ellipsoid of revolution, and of those minor ones which produce the mountains, the cavities of the ocean, and the other irregularities visible on the surface.

The essential and simple condition to the ellipsoid of rotation is, that the whole earth, or a certain considerable portion of the surface, namely, that part contained between the surface of the terrestrial spheroid and the sphere inscribed within it, should at one time have been fluid. It does not possess sufficient conditions at present; its parts being actuated by forces of cohesion too considerable, to allow it to assume the forms depending on statical pressure and the velocity of its revolution.

It remains therefore to be explained by geology, how that form which has been shown, under certain irregularities, to exist, is connected with the geological history of the earth. Hence the doctrine of a primeval state of fluidity in the whole, or in a large portion of the globe, has formed a fundamental part of the hypotheses of all geologists; though one party has attributed that fluidity to water and the other to fire.

Another suggestion has however been offered; namely, that the mass of the earth is at present fluid, and that all the solid and superficial parts invest it, as if floating on the surface. The flotation is not inconsistent with mathematical laws; but unless a very considerable depth be assumed for the solid portion, this supposition does not explain the greater variations from the true statistical figure, which, if they should really exist, it will have overlooked.

The present state of the earth affords, not only no indications of universal fluidity, but not even any mark of a past fluidity of all its superficial parts. Further, chemical considerations show, that the insoluble nature of the rocks or their constituents is such, that no quantity of water can at any time have been present, sufficient for that purpose, without producing effects incompatible with the general motions of the solar system; since, on the most favourable of the two suppositions, the water necessary to this purpose would exceed the earth in a ratio greater than that of 4 to 1. Nor does chemistry provide any method of abstracting or destroying that water after its supposed duties had been fulfilled. If, on the contrary, we attribute the fluidity of the earth to fire, the difficulty, if it be supposed one, may, in the same way as in the case of solution in water, be diminished by limiting the fusion to the same crust covering the inscribed sphere, of which the diameter is nearly coincident with the length of the Polar axis. But though it is demonstrable that the great mass of the superficial rocks has been deposited from water, that fact, as of more recent date, is not incompatible with such a theory. Adopting this supposition, we must however explain the loss of that heat which must have been extricated on the cooling of such a mass. For this purpose, recourse has been had to the chemical changes and combinations in which heat is absorbed or becomes latent; and if this explanation is attended with difficulties, it is conceived that the doctrine of radiation offers a satisfactory solution.

That the planetary bodies, as well as the solar ones, must and do radiate their heat into free space, is a doctrine to which we cannot well refuse our assent; and thence the mean temperature of the ambient void, imperfectly indicated by that of our own polar regions. It is almost unnecessary to say, that if the earth was even that heated body which this theory supposes, it must, like a solar one, have radiated its heat until its surface, at least, had subsided to that temperature; omitting here any consideration of the solar action. Hence, the cooling of the earth from a high state of former heat, is not the impossibility which has been imagined; while the laws which regulate its progress through conducting bodies, would permit the coexistence of the present superficial temperature with any imaginable one in its interior parts.

But another hypothesis to explain the statical figure of the earth has been proposed, founded on the changes which it is actually undergoing, and appears to have undergone, from the waste of its surface and the protrusion of rocks from its interior parts.

If it were a solid sphere surrounded by an ocean of a limited depth, and that a certain movement of rotation became communicated to it, the water on the equatorial regions would rise so as to produce a zone, while the polar surfaces would become dry. Thus the division of sea and land would form two circumpolar continents with an intermediate ocean. If again it be imagined that from any causes of destruction, the exposed land were worn down, and its ruins carried forwards into this sea, a series of strata would be formed, inducing a disposition to the statical figure of the solid earth. The ultimate result of this change, is the ellipsoid already mentioned. At the same time, the unequal hardness of the originally exposed land, would be the cause of irregularities in its new form, similar to those existing on the dry surface of this globe. Under similar circumstances, be the actual figure of the globe and its irregularity what they may, there would be the same constant approach to the statical figure; nor could any rest take place till, by the general balance of forces, this ultimate form had been assumed. The only figure calculated to resist the effect of gradual changes, is that which is produced by the changes themselves.

The result of this reasoning is, that any spheroid of solid matter, which is partly covered with water, and is subject to those actions of waste above it, accompanied by processes of reconsolidation beneath, which exist in the earth, must ultimately assume the form of an ellipsoid of rotation, as perfectly as if it had been fluid throughout. Time alone is necessary for that perfect change; and it is supposed that we have no reasons for limiting the time through which the globe of the earth has, in some form or other, existed.

It is esteemed another argument in favour of this view, that while it explains, equally well with the hypothesis of fluidity, the general statical form, it also accounts for those irregularities, great as well as small, by which that is supposed to be modified. Revolutions of the surface occurring during the system of progressive change, counteract the ultimate result: while original inequalities of figure or of hardness, and greater power of resistance in certain parts, will also explain why, even during so long a period of time as may safely be granted, a more perfect general figure has not yet been obtained. The hypothesis of universal fluidity is said to be comparatively deficient; as, under this theory, no deviation from the statical figure, beyond those irregularities which constitute the immediate surface of the land, ought to have occurred.

A plausible hypothesis is not however necessarily true. Geological readers need scarcely be informed by whom this one was proposed; and it is not difficult to perceive the bias by which he was influenced. The presumed irregularities of the earth by which it was chiefly supported, are by no means ascertained. Mathematicians are far from satisfied respecting those deviations which I have noticed in this chapter, and on which it was the business of this hypothesis to dwell. The apparent irregularities of the meridians which have been deduced from observations in England or elsewhere, have not convinced astronomers that the earth is that irregular body which it conceives.

CHAP. IV.

On the general Disposition of the Surface of the Globe.

To CONSIDER, in all its details, the subject which forms the title of this Chapter, would be to trespass on the province of Physical Geography. But this department of Geographical science is in so many points implicated with geology, that it is impossible to avoid bestowing on it some consideration, although it cannot here be allowed a large space. If it is the business of the Geographer to trace the outlines of coasts and islands, and the directions of chains of mountains, it is that of the geologist to assign the positions and courses of the strata by which these have been modified or determined. Geology teaches the Geographer the nature of the changes by which lakes have been obliterated, by which rivers have changed their courses; it conducts him from the mountain to the plain, and shows him why that which was once sea is now firm land. In contemplating the rocks of Niagara it foresees a period when that torrent shall no longer plunge into the abyss below; and, in viewing the fires of Ætna, it detects the causes that erected the splendid colonnades of Staffa and Sky.

Of the Distribution of Sea and Land.

The most prominent circumstance in the surface of the earth relates to the general distribution of sea and land; and if that which appears to be the next remarkable feature, namely, the inequality of its elevations, be considered, it is plain that these are merely convertible propositions. With the forms of continents or the distribution of islands, geology can therefore have no concern, otherwise than as these are necessary consequences of differences of elevation, and are dependent on Geological causes.

Those who have treated of physical geography, have attempted many generalizations on this subject; but these seem to establish no principles, and they add nothing to that knowledge which may be derived with much more ease from the inspection of a terrestrial globe. If we examine what has been written on the directions of Capes and coasts, or the correspondences of bays and head-lands, we shall find nothing which can be referred to any general law. Whatever modifications exist, they are particular cases; and, wherever they occur, they depend on the altitudes and directions of elevated land, or on the positions of the mouths of rivers; circumstances which, as far as they are subject to any rules, will be examined hereafter.

The greatest elevation of land has been measured; but its greatest depression, being concealed by the sea, is beyond the reach of our instruments. Thus we are still ignorant of the quantity by which the least extreme diameters of the solid earth differ from the mean mathematical ones.

But by La Place's computation, founded on the theory of the tides, the mean depth of the sea must be four leagues. If we admit that to be only its extreme depth, and that the height of the Himálya ridge is 5 miles, we shall have a minimum diameter differing from the greatest by $\frac{1}{470}$ part, nearly, of the mean. But whatever the maximum differences of such inequalities are now, it is certain that they have once been much more considerable. This is proved by the degradation of mountains, and by the formation of submarine deposits; circumstances by which there is a constant tendency to restore the mean diameter throughout the globe, and thus to afford the means, not only of producing at some future time a spheroid more regular than the present, but of conducing towards the perfection of that figure treated of in the last Chapter.

Little as we know respecting the form of the bottom of the sea, it is still certain that it possesses, like the surface of the dry land, its mountains and valleys. The soundings of mariners, limited as they yet are, have proved that which might have been inferred without experiment. Between Greenland and America, this shoaling of the water produces that submarine hill on which the icebergs ground and rest so late in the summer as to impede the navigation across the bay when it is free along the shores. On the coasts of Newfoundland, and in many places round our own islands, similar elevations are the favourite resorts of Cod and other fish, producing the cod banks of fishermen; while those hills, elevated to a still greater altitude, form the islands that are every where scattered over the ocean. The sounding line often detects the forms and extent of these when the depths are not too considerable, and discovers, that as on shore, they vary in the steepness of their acclivities. In the same way, vallies are found depressed beneath the general surface of the bottom; as are the beds of our lakes on the land: and on the coast of Shetland, these vallies, presenting a sudden depth of seven hundred feet, or more, are the favourite haunts of the Ling, as the hills are of the Cod.

Were it not known to be the fact, it might easily be concluded that the low lands were bounded by shallow seas, and that deep seas accompanied mountainous shores. The soundings of mariners have long since proved this to be the case; and it has often been turned to use in navigation and pilotage. The frigate that dares not carry sail on the coasts of Holland or England without the constant use of the lead, stands on fearless through the narrow fiords of Norway, raking the cliffs with her yard arms. Where the æstuaries of rivers enter, there also shoal water is to be expected; and this state of the bottom often extends to a great distance, as at the mouths of the Oroonoko and the Plate river.

If, lastly, the submarine surface be examined, it will rarely be found naked and rocky; and only so in those places where it rises into acute peaks or sudden elevations. Like the land, it is covered with an alluvium: forming the soil, in some places, on which submarine vegetables grow, where shell fish reside, and where fishes deposit their spawn; but answering the far more important purpose of laying the foundation of future terrestrial strata.

Of Mountains and Valleys.

The most conspicuous objects on the land are the mountains; those repositories in which the Geologist reads, in the most intelligible language, that which it is his object to learn. Much has been written respecting their arrangement and distribution; and, with the usual morbid love of generalization, much has been laid down which is little better than imaginary. The reader must not expect to find here a technical language which is founded on attempts to define that which is indefinite: a minute logic dealing in words rather than things, and producing the usual results that have always been produced by unideal phraseology.

The elevations of mountains present the first palpable object of curiosity, and one which has assuredly met the full consideration which it deserves, excepting as it may be esteemed a mere fact. This is, however, a natural consequence of the awe produced by the bulks and forms of these, by the desolation and nakedness in which nature is here displayed, and by the picturesque effects or the terror consequent on the atmospheric phenomena in which they abound. It is here that man can best compare himself with the objects around; where he becomes most conscious of the insignificant spot which he occupies on the earth.

The highest elevation of the globe is that of the Himálya mountains, which form the sources of the principal rivers of India. I will not here do more than allude to certain similar elevations lately asserted to exist in Peru, as they have not been verified. The altitude of the Himálya appears to have been at length satisfactorily determined, attaining to 25,749 feet. But as this is a subject of a numerical nature, it is best to select, from the tables often published, a few of the most conspicuous elevations throughout the world. They whose curiosity on this subject extends further, may refer to records that are in the hands of every one.

	FEET.
Snowdon, the highest of England - -	3,568
Scotland. Ben Lawers, Ben Nevis, and Ben Mhuc Dhubh, above - -	4,000
Macgillicuddy's Reeks. Ireland	3,404
Iceland. Snæfell	6,860
Norway. Swicken	6,658

	FEET.
Sweden. Areskutan	6,180
Hanover. Brocken	3,690
Bohemia. Ochsenkopf	3,980
Switzerland. Mont Blanc	15,680
Tyrol. Oertler Spitz	15,430
Saltzburg. Ostelle	12,800
Hungary. Lomnitz; Carpathians	8,640
Spain. Pic Blanc. Pyrenees	10,205
France. Canigou	9,290
Italy. Etna	10,963
—— Vesuvius	3,900
Turkey. Lebanon	9,520
—— Ararat	9,500
Greece. Olympus	6,500
—— Ida	4,960
—— Athos	3,353
Thibet. Himâlya	25,749
Teneriffe. Pico	12,236
Caucasus. Elbrouz	16,700
—— Mginwary	14,400
South America. Chimboraço	20,909
Jamaica	7,431
North America. Mount St. Elias	12,672
North Pacific. Mouna Roa	12,700

The arrangement, or distribution, of mountains, whether as it relates to the general surface of the earth or to particular tracts, has been a subject of much discussion, and of dissertations not a little tainted by error and prejudice. An imaginary regularity of extended chains from S. W. to N. E. has not only been stated with as much confidence as if it really existed, but it has even been asserted that all the great chains of the earth preserved a general parallelism, and lay between certain fixed points of the compass.

This fact, real or imaginary, but sufficiently real in certain places, has been connected with another circumstance, namely, that the strata themselves held the same general directions, and that, at the same time, they were parallel to each other. Within small distances, parallelism and bearings on the compass will doubtless agree; but if it be attempted to generalize this agreement for much larger spaces, or for the whole globe, the want of thought which dictated this conclusion becomes detected. It is evident from the doctrine of the sphere, which has been overlooked in these reasonings, that two strata with north bearings, for example, however parallel for a short distance, will intersect at the poles if indefinitely prolonged; and that, if meeting from a quadrant's distance, they will cross each other at right angles. It will be shown in another place, that the directions of strata, even for small spaces, do not always regulate those of mountainous ridges; although there is unquestionably, in many parts of the world, a great consistency in the elevations, and consequent bearings, of the stratified rocks.

With respect to the consistent parallelism, similarity, or direction, of extended elevations, we have Saussure's authority for the great irregularity of the Alps, and Ramond's for that of the Pyrenees. The Cordillera of Mexico is an elevated platform, never less than 5000 or 6000 feet in height; nearly parallel to the western ocean, and the higher summits are irregularly placed upon it. In India, the Ghauts flank the western side of the peninsula, and their summit is an irregular platform. The high ridge of Peru is not a chain, nor does it even resemble that of

Mexico; since the elevated summits form its crest, and it is intersected by deep transverse vallies. The Norwegian mountains, in the same manner, have a northerly direction. Thus there are exceptions sufficent to prove that the extension of mountain chains from N. E. to S. W. is imaginary. But to examine the real directions of all the ridges and elevations of the globe, is the business of physical geography. It is sufficient for the present purpose to state the exceptions, that no fallacious geological conclusions may be drawn from an ideal and universal direction. And when it is stated that, in the Pyrenees, the direction of the strata is E. S. E., in Sweden and Finland S. and S. S. W., in Scotland S. W. and S. S. W., in Mexico S. E., and in the Allegany S. W., it will not be necessary to produce a greater approximation of examples.

With respect to the particular distribution of the subordinate parts of any group of mountains, whether extended or otherwise, it is subject to irregularities that admit of no rule; though it has been the source of much trifling discussion and of many frivolous distinctions. If mountains had been formed and disposed every where by some invariable law, and again acted on and demolished by other invariable laws, such enquiries might claim the merit attached to the investigation of truth, and lead to eventual utility. It is almost superfluous to say that neither of these is the fact. They consist of various rocks, of unequal qualities, unequal forms, and unequal distribution; and they have been acted on so as to have lost their original forms, by causes which, in the ordinary acceptation of the term, have been regulated by chance. That an irregular conoidal mountain rising from a plain, should possess a summit higher than all the surrounding ones which form its sides, is a mere repetition of the proposition itself; and that, among many eminences, one or more should be higher than the others, is an arrangement that might be expected. If rivers run down the sides of a lengthened declivity they must in time produce ridges at angles to its course; and where these rivers themselves bend at angles, their boundaries must correspond, or a salient angle must be opposed to a re-entering one. These are truisms which add nothing to knowledge; and the discussion of them may be left to those in whose eyes they possess estimation.

As the direction of mountain ridges has been said to bear a certain constant relation to the meridian, so it has been asserted that the steepest declivities and precipices always respected one point of the horizon. A predominant North-west declivity has been supposed, for example, to exist in Scotland; and indeed those who have directed all their mountain chains to the North-east, have placed their precipices to the North-west. By comparing testimonies, the state of the facts will soon appear; and it will then be time enough to shew what, from geological considerations, they ought to be.

Bergman first appears to have remarked that, in North ridges, the western declivity was steepest, and in east ones the southern; but, in quoting his examples, he has overlooked the exceptions. The observations of Buffon, De La Metherie, and others, rest on similar grounds; and, according to Forster, the south and south east sides are the steepest. In the Crimea, in the Hartz, in the mountains which separate Saxony from Bohemia, and in the Carpathians, the southern declivity is the most rapid; as is also the case in Guiana, according to Condamine. The chain that separates Norway from Sweden is steepest on its western side; as are the chains of the Oural, the Ghauts, the Cordillera, and the mountains of Syria. The mountains of Kamtschatka are, on the contrary, said by Pallas to be steepest on the eastern sides; while those which separate Silesia from Bohemia, the Meissner in Hessia, a considerable portion of the Pyrenees, and some of the mountains of Armenia and Caucasus, are described as being most abrupt in their northern declivities. In Scotland, there is no predominant tendency to be traced any where; although the cause from which such tendencies do actually arise, exists in that country.

Such are the facts; and on examining the causes, this irregularity is justified. It would require four arrangements to render an universally prevalent declivity true; and neither of these exists. The ridges ought to have a predominant direction, that direction should be the same as that of the strata, the dips of these strata should be always to the same point of the horizon, and all mountains should consist of stratified rocks.

It is only requisite to determine the inclination of the strata in stratified mountains, and their greatest declivities will appear. It is necessarily found at the side of the elevated edges; as, on this quarter, they are most subject to disintegration. There cannot therefore be a predominant declivity, either in the cases of horizontal or vertical strata, or where the direction of the strata crosses the line of the ridge; as it does in some cases. It could not be found universally in one quarter of the horizon, unless all chains had the same direction and all strata the same inclination; nor could it even bear a respect to the directions of individual ridges, unless the inclination of their strata was, in all, analogous. And lastly, as granite and trap are not stratified, they can be subject to no laws; so that their precipitous sides must be determined by a variety of incidental causes.

But some causes of great moment having been assigned by great names, for facts which seem to have no existence, it is necessary to notice them, lest this subject should appear to have been treated too lightly. Forster and Pallas have explained the imaginary general declivity of mountains, by a system of currents flowing in that direction; and Kirwan, finding two declivities to contend with, has been compelled to invent two diluvian torrents. It is admitted that there may be more alluvium or soil on one declivity of a mountain than another, and that this may be a general feature in particular districts, as it is said to be in some part of Scotland. But all which this naked facts proves is, that the alluvial soil finds an easier resting place on the gentlest declivities, or that it accumulates in greatest quantity at the foot of the steepest. It is merely a collateral and necessary effect of the forms of hills; and whatever some examples may prove respecting great and general currents of water, there is not a shadow of evidence of their power to demolish mountains. Of those currents which have taken place on the surface, we must seek for the proofs in other facts than the precipitous faces of mountains.

It has been often remarked, that the highest mountains lie in the lower latitudes, and that as we leave the equator, the elevations diminish. But this remark is not sufficiently free of exceptions to serve any useful purpose; nor does it appear to be in any respect connected with those peculiarities with regard to motion or general figure, by which the equatorial are distinguished from the polar regions. But it is useless to pursue questions of this nature, which at present seem to lead to no useful geological results, but which may be recorded with propriety among the independent facts that belong to physical geography.

The term mountain is of very vague signification; and, however generally limited to a certain scale of altitude, that scale is rather regulated by comparison than by a fixed rule. The hills of Tweedale would excite far different sensations in the plains of Poland, and at the foot of Ætna; and, to the shepherds of the Valais, the elevations of Gogmagog would be invisible. Of a conventional and variable term, no definition can be given; nor is it required.

The sudden ascent of a very high mountain from a plain is rare; and, except in the case of some of the volcanic mountains, it is scarcely known. As mountains gradually descend in altitude they become hills; and thus it is also a necessary consequence, that hills formed of elevations gradually diminishing, should intervene between the higher lands and the plains. The arrangements of such hills with respect to the higher mountains which they accompany, is partly regulated by the nature of the rocks and the position of the strata, and partly by the water courses which have furrowed them and removed the materials that once connected them more intimately together. But it is chiefly in the higher elevations that the asperities produced by naked and protruding rocks are found. The effects of the atmosphere, the power of rain and frost, far most active in these elevated regions, conspiring with the force of gravity, demolish that which can find no resting place on the steep declivities, and thus leave naked and bare those pinnacles and precipices which, with different views, form alike the study of the painter and the geologist.

Hence the lower elevations are rarely marked by conspicuous asperities; and, except under peculiar circumstances of climate or exposure, or when formed of certain indestructible rocks, they seldom display the nature of their structure or contents. More commonly they are covered with that alluvial soil which finds so firm a resting place on them; and thus their outlines are rounded and tame, while being at the same time favourable to the uses of man, they are covered with vegetation. If ever, in the lower hills, the naked rock is displayed, it will be found at those places where the elevated edges of the strata lie, and where the geologist finds, if with difficulty yet most securely, those indications of the internal structure of a country which are of so much easier access in more elevated regions.

It is scarcely necessary to say, that between the spiry and rugged mountain and the flat vacant plain, every degree of altitude and every form of undulating surface may exist. But even plains are sometimes considerably elevated above the sea; although it is more usual to find them at low levels. In extent, they vary from the enormous tracts of Asiatic Russia and the sandy deserts of Africa, to the narrowest limits of those which diversify the undulating lands of England. In some instances, their forms depend on the horizontal or even positions of the strata on which they lie; but, in many, they are the produce of alluvial deposits, resulting from the joint action of the sea and rivers.

Every interval between two hills forms a valley; and thus the extent, the forms, the dispositions, and the depths of vallies, are counterparts of the corresponding circumstances in the elevated lands by which they are determined and bounded. In the most abrupt mountainous regions, the vallies are consequently deep, with steep sides, often precipitous or rocky. In these cases, they are frequently also very narrow; the opposite sides meeting below, without an intervening flat; and forming that variety well expressed by the Scottish term glen. If the bottom has an uniform declivity, the valley then give passage to a permanent stream or occasional torrents; if curved, it becomes the seat of a mountain lake.

Where the declivity of the bottom of such a valley is gentle, and especially where the breadth is considerable, alluvial matters in time accumulate on the lower parts, and thus a plain is formed within it; producing that peculiar variety of shape distinguished by the Scottish term Strath. Lastly, in lands of lower undulation, the valley expands until its peculiarity is at length lost in the gently undulating plain. It will be a subject for future inquiry, how far the forms or the existence of vallies have been the consequence of the action of rivers.

Of Springs, Rivers, and Lakes.

The nature and existence of Springs, as forming the origin of rivers, is one of those circumstances in the physical history of the earth which is peculiarly connected with the phenomena of Geology. But it is a very obscure subject, and one respecting which we can, in many instances, scarcely form a rational conjecture.

It is a common remark by writers on this subject, that springs abound most in mountainous countries, and that they break out at the feet of the declivities. This assertion is far from being generally true; particularly if we include those which, from the peculiar nature or declivity of the surface, are unable to reach it, but which are found in penetrating some particular stratum, giving rise to the wells commonly used for procuring water in countries of low elevation. Throughout the whole mountainous range of Scotland, springs are rare, nor does any one of great magnitude exist; all the great rivers of that country being collected originally from those superficial mountain torrents that proceed from the hourly rains which fall on their innumerable elevated sources. In the comparatively low lands of England, on the contrary, they abound; while those of the Thames and other rivers of that country, are testimonies of the size and power with which they break forth, far from the declivities of mountains.

There is more truth in the observation that, where they do abound, they will be found to regard the inclinations of the strata; breaking out at the foot of that declivity which is formed by the dip. Thus well-diggers are directed in their search; as they also are by observations on the rocks or substances, of which the several strata are formed. In this case, the water finds its way, chiefly, between two strata, of which the lower is impermeable to it. It has been often remarked that some particular rocks abound in springs, while, in others, they are rare or wanting; and this, it will shortly be seen, depends chiefly on the disposition or existence of fissures. It has been said that they were common in the trap rocks; but, in Scotland, that rule does not hold good. Thus also, in many parts of the Gneiss of Scotland, springs are scarcely known; whereas, in the island of Guernsey, in the same rock, they burst forth in a thousand places. Such assertions must be added to the endless examples of fallacious generalization. Throughout our own island, it may be observed, that they are not in the sea. Lakes are, in some instances, the immediate sources of rivers, but rather in a geographical than a physical sense; as they are themselves fed by streams, and ought rather to be considered as basins interposed in their courses. It is not in Loch Tay that we must seek the source of that river, but in the innumerable streams which contribute to swell the Lyon and the Tumel, the Isla, the Garry, the Lochy, the Dochart, and the Almond.

As rivers which are fed either by snow or rain vary irregularly in summer and winter, so those that are much indebted to snows, as the Wolga, are fullest in the early heats of May and June. From the same causes, many mountain streams in Peru and Chili, flow only in the day. A clue to discover the origin of rivers whose sources are unknown, is thus sometimes offered, by considering the places and times of periodical rains or summer thaws, and by comparing these with the increase of their waters. This species of reasoning has often been applied to the questions, yet obscure, which concern the origin and courses of the Niger and the Nile. From these periodical rains or thaws, arise the inundations which are the sources of fertility to Egypt, and which are conspicuous in the Indus, the Ganges, the Plata, and many other rivers throughout the world.

The direction of rivers is necessarily regulated by the form and elevation of the lands whence they arise and through which they flow; and, as far as particular tracts are concerned, the division of streams to opposite directions, points to the highest level. The particular history of these courses is often interesting, but would lead beyond the bounds to which the present sketch must be limited. The length of the courses of some of the larger rivers of the world is very great;

and, among these, the great rivers of America are conspicuous. The course of the Ganges is estimated at 2000 miles, that of the Nile at 2400; but the Orellana is said to run 5000 miles before it reaches the sea. Among the larger rivers of the earth, may also be enumerated the Oby, the Jenissei, the Saint Lawrence, the Amazon, the Plata, the Lena, the Amoor, and the Hoanho of China. The breadth of some of these is no less extraordinary than their length; the Orellana being sixty miles broad at its exit, and the Plata ninety; the mass of water which they bring down freshening the ocean to great distances from their æstuaries.

The æstuaries of rivers vary according to the form of the land which they last quit; and, in the larger, are regulated by their own actions. It is only in rapid declivities in general, or where they have made more deep sections in the land, that they open by one mouth. When they deposit much sand and mud, they not only form various openings, but these are subject to changes, both in number and position; circumstances which often render their entrances difficult and dangerous. Thus the Danube opens into the Euxine by seven mouths; while the Wolga terminates by not less than seventy. Of changes of this nature, the Nile is an example; as it is recorded by antient writers to have once entered the Mediterranean by the Canopic branch alone; whereas it now opens by seven distinct æstuaries.

All rivers do not however terminate in the sea; but many are lost in lakes, as in the Caspian and the Dead sea, whence no corresponding streams find an exit. Some vanish in marshes or sands, an instance of which has lately been thought to be discovered in New South

Wales; and this has been imagined to form also the termination of the Niger. In a few instances, small rivers are found to sink into the ground and disappear; while, in others, after a subterranean course, they rise again. The action of rivers in changing or influencing the forms of the land, will be considered in another chapter.

The description of valleys has already explained the origin of lakes, which must necessarily exist wherever the middle of such a valley is considerably lower than its lowest extremity. Thus they are necessarily most frequent in mountainous countries; although very extensive collections of water of this nature are also found in plains. They may of course exist at any elevation above the sea; and, in some rare cases, of which Loch Ness is an example, their beds are even depressed below its level.

In general, lakes both receive and emit a river, being depressions in the course of its stream; or else, receiving many streams, they emit only one. The most conspicuous lakes of this nature in Europe are that of Geneva, traversed by the Rhone at an elevation of 1134 feet above the sea, that of Lucern, at 1392, traversed by the Reuss, that of Constance, at 1089, giving passage to the Rhine, those of Brientz, Thun, Zürich, and the lakes Como, Garda, and Maggiore in Italy. In America, a whole chain, consisting of the Lakes Superior, Erie, Ontario, and Huron, is traversed by the Saint Lawrence; and, in our island, this is the character of all which are in any way remarkable for their extent.

In a few instances, lakes receive rivers without emitting any; and, of these the most remarkable is the Caspian, just noticed, into which the Wolga and the Ural flow, and the Dead sea: and, in all of them, it must be supposed that the waste by evaporation is equivalent to the average supply of water.

Some lakes of less importance are found to emit rivers without previously receiving any. The Seliger lake is the source of the Wolga; and the Hoanho and Kiam of China, are said to arise from similar sources. But it is more probable that many of the examples quoted by geographical writers, are the receptacles of insignificant alpine rills, like Loch Spey in our own island.

The last modification of lakes consists of those which neither receive nor emit streams, such as those of Agnano and Averno in Italy, which, with some others described by authors, appear to have been the craters of volcanoes. In such cases, we must conceive that they contain internal springs adequate to the supply of the mean evaporation.

Subterranean pools and lakes have been described by authors; and, of these, Pen-park hole, in Gloucestershire, is an example in our own country. These are sometimes evidently formed by irregularities, or occasional depressions, in the courses of the subterranean rivers so often found in limestone countries. Where they are found to exist in the volcanic regions of America, supplying, as is supposed, the fish sometimes ejected by these volcanoes, they probably occupy caverns of volcanic origin.

CHAP. V.

On the general Outlines, or the picturesque Characters of Rocks.

As it has been frequently asserted that the geologist may derive essential assistance in his investigations, from the general features or picturesque characters of rocks, and as some rapid and superficial travellers have even ventured to describe tracts of country from distant observations, made, often, in haste, and sometimes by means of a telescope, it will not be here misplaced to inquire what value is due to such observations. After reading the few remarks that follow, the student may determine how far he may avail himself of this assistance, and where a reliance on it will mislead himself and deceive his readers. That it may serve many useful accessary purposes, or occasionally convey valuable hints, is unquestionable; but it will be seen, that without great precautions, it will more frequently contribute to deceive than to instruct the observer.

To profit by this class of observations, it is, in the first place, requisite that the geologist should possess the eye of an artist and the practice of a landscape painter; without which he will be a very imperfect judge of those minute variations in the picturesque characters of rocks, or of the tracts of land which they may form, that often indicate essential variations in their nature and disposition. It is next necessary that the observer, thus qualified, should have been long practised in the investigation of rocks in the large masses in which they occur in nature, in different countries, and under every variety of form and distribution; and that he should, further, have studied for himself the picturesque forms with which they are usually associated. Neither descriptions nor drawings can convey this instruction; and the advantages to be derived from these circumstances are therefore such as to be nearly limited to those who will probably have little occasion for them. To the geological student, this knowledge cannot be communicated; and it is of the very essence of his imperfect attainments in the science, to be unable to turn this delicate and precarious species of information to use.

I need not anticipate the difficulties and the uncertainties in this class of observations, which render a reliance on it objectionable in a general view; but am at the same time willing to concede, that in the hands of an expert and practised geologist, it may often be rendered a useful accessary; furnishing valuable hints respecting circumstances which it is unable to determine, but which may often direct the steps and shorten the labour of the observer. But to render this proceeding really useful, and at the same time safe, these observations must not be extended beyond particular countries, and must often be limited to very narrow districts. Although any particular rock may be found to present various picturesque aspects in different countries or distant tracts, it is occasionally sufficiently consistent, in one place, to enable the geologist to extend, by his eye or his telescope, those observations which he has elsewhere made by his hammer and his hand. Yet he will, in the following remarks, find abundant reasons for not trusting further to that aid than is absolutely indispensable: and, to the student in geology, no safer rule can be given, than that he can be certain of nothing which he has not touched. His eye may deceive him, but he will never be misled by his hammer.

It is a common remark, that granite occupies the highest parts of a country, and that it produces those pinnacled and serrated summits so well known to those who have visited the Alps, or have, in our own country, seen the mountains of Arran. Yet a geologist who shall trust to this feature as characteristic of granite, will be deceived much oftener than he will form a correct judgment; as that rock presents every variety of outline, and as many others assume the spiry and serrated form. The mountains about Loch Etive in Scotland, are characterized by the simple conical outline, which is particularly marked in Cruachan; and they are unvaried by a single serrature or pinnacle. The extensive ridge which surrounds the sources of the Dee, forming the loftiest tract of mountain land in Britain, presents a series of heavy, rounded, elevations; on which, if we except a few of the cairns that are scattered over Ben Avon and others of the group, not an irregularity exists to indicate the nature of the rock, which is nevertheless a continuous mass of granite. In Cornwall, in Galloway, and in Sutherland, it offers the same uninteresting aspect; while, in many parts of Aberdeenshire, it occupies the lowest grounds, presenting large tracts of a surface as level as that which has been supposed to characterize districts of secondary rocks.

Thus the observer who may be so far induced to trust to the serrated and spiry outline, as to exclude from granite, or to neglect, those tracts which do not present this feature, will deceive himself and impede his own progress. He will commit similar errors at every step, if, on the other hand, he shall resolve to consider as granite, every distant hill that is crowned with pinnacles and diversified by an acute indented outline. No eye can distinguish between the serratures of the Arran mountains and those of the Cuchullin hills, although the former consist of granite and the latter of Hypersthene rock. The gneiss of Harris often presents features exactly similar; nor would any thing short of manual examination convince the observer, that the pinnacles which rise along the spiry ridges of Kea cloch in Rossshire are formed of sandstone. Even limestone is known to be occasionally disposed in the same manner; and the innumerable spires of Montserrat in Spain are the produce of a conglomerate rock.

Even in the more minute features, granite cannot always be distinguished from other rocks, although under the very grasp of the observer; if, confiding in the accuracy of his eye, and relying on his own imagined experience, he shall trust to that alone. In a thousand places in Aberdeenshire, the external forms of the masses, the cairns, and the loose blocks of granite and gneiss, are so exactly alike, that the geologist, who is even long experienced in that country, may traverse them and examine them in every direction, and still remain unsatisfied till he has brought them to the test of his hammer. The very cairns of granite, the piles of prismatic or rounded blocks, are mimicked by sandstone so as to deceive the finest eye, in many parts of the Western coast of Scotland; as its huge curved continuous beds are by the Hypersthene rock of the Cuchullin, and by the greenstone of the Corstorphin hills. But I need not accumulate more examples in this rock: enough have been adduced to show that no combination of the experience of the most practised geologist with that accurate eye for form and character which distinguishes the painter, will exempt the observer from the duty of a careful manual examination where granite is concerned.

It has been so often said that the trap rocks are characterized by the scalar outline from which their Swedish name, now adopted by us, has been derived, that it is necessary, for the sake of the geological student, to examine into the truth of this assertion. That outline does unquestionably occur ; but it is limited to those examples where these rocks exist in the form of beds, either horizontal or nearly so ; as in the little Cumbray, and in some parts of Sky, of Mull, and of the neighbouring islands. But as this peculiar outline is produced by the successive and unequal loss of portions of such beds, it is evident that it may occur in any stratified rock disposed in a similar manner; provided its fracture is in a direction nearly vertical to the strata. It will therefore probably be found in horizontal sandstones; although at this moment no very well marked instance occurs to my recollection. But a thousand instances may be quoted, where the trap rocks deviate from this outline; while the several picturesque characters which they exhibit are so infinitely varied, that no experience and no eye, are capable of pronouncing on their nature from a distant view. The marked granitic character of the Hypersthene rock of Sky was already noticed; and, with respect to the syenites, porphyries, and claystones, which form the interior hills of that island, they are undistinguishable from the granite which constitutes the mountains of upper Lorn. In Fife and in Perthshire, in the ridges of the Ochils and the Sidlaw, it is utterly impossible to conjecture the place of any one trap rock, from the outline or general aspect of the ground. Yet the differences of the superincumbent and subjacent rocks can not well be greater; as the latter consist of argillaceous schist, of the lowest red sandstone, of the lowest secondary limestone, and of the coal series. Every possible variety of outline, conical, undulating, or flat, will be found, somewhere or other, attending the trap rocks of Scotland; and he will be a fortunate geologist who is not obliged to surround, and almost handle, every mass of these rocks, before he can determine their nature or assign their limits. The formidable cliffs of Saint Kilda present a diversity of characters and aspects differing from those of any other analogous rocks in this country; and, in Sky alone, there may be found every variety of disposition and outline which is displayed in nature. Even the columnar form which has been supposed to indicate basalt, is not limited, either to that rock, or to the greenstones; since it exists in the claystones of Mull, Rum, and Arran, in the Syenite of Ailsa, in the pitchstone of Egg, and even in the sandstone of Dunbar.

If granite and the trap rocks, which have been supposed to possess such marked and characteristic features, are thus subject to variations in their picturesque and general forms, still less is it possible to rely on this guide in examining the various stratified rocks of the primary and secondary classes. If in a few situations, as in Coll, in Rona, and on the west coast of Rossshire, gneiss may be recognised at a distance by the insulated and naked grey rocks every where protruding through the soil, in numerous others, as in Perthshire and Aberdeenshire, it cannot be distinguished from the granite which it accompanies, far less from the micaceous schist and quartz rock with which it is so often interstratified. In itself, like granite, it presents every possible form of outline; sometimes displaying broken precipices and rugged summits, at others, being every where covered with soil, and forming rounded smooth hills or flat and undulating low tracts. It is fruitless to pursue this inquiry through the remainder of the strata. In micaceous schist, in quartz rock, and in argillaceous schist, the same uncertainty and confusion of character are sufficiently obvious, and will not fail immediately to be perceived by those who, at the commencement of their progress, have been induced to trust to so fallacious a guide.

If, among the secondary strata, the limestone of Dovedale is distinguished by its pinnacles and castellated forms, so is the far different calcareous rock of Istria and Dalmatia, and the sandstone, called quadersandstein, of the Germans. Even the same disposition is found conspicuously to prevail among the conglomerates which belong to the lowest red sandstone; as was already remarked in speaking of the pinnacled form of granite. Among the greater number of the other secondary strata, it would be in vain to look for picturesque distinctions; as vain as, in most cases, it would be to try to distinguish them from the primary rocks, when the same general undulations are found in both, and where both are equally covered with alluvia and soil.

It is unnecessary to say more on this subject. The purpose of these remarks is not to deprive the experienced geologist of the advantages which he may derive from a correct and practised eye; since they are far from inconsiderable, if the precautions inculcated at the beginning of this chapter are regarded. Their object is to caution the student against that confidence which is, too often, both the result of indolence and its excuse; and to inculcate on him the necessity of establishing habits of careful and minute observation. An eye for the physiognomy of a country must be acquired by practice; and it will never be acquired by him who shall attempt to find a royal road to that rugged eminence which has never yet been attained without patient study and toilsome exertion.

CHAP. VI.

On the general Distribution of the Materials which constitute the Earth.

The superficial parts of the earth, which are all to which we can have access, whether by observation or inference, are formed of indurated and of loose materials; the first constituting rocks, and the latter being the various alluvial substances which, no less than the former, are objects of geological investigation. In the following chapters, the various details, whether of a general or of a particular nature, namely, those which relate to the substances themselves, and those which have a reference to the constitution of the globe, the origin of these materials, and the various revolutions they have undergone, will be considered in the several points of view which may appear necessary. The object of the present is to give a sketch of the most remarkable distinctions among rocks, and of their general distribution as constituent parts of the earth.

Observation has shown that rocks may be divided into unstratified and stratified: or into those, of which the forms are irregular, and those which are disposed in successive beds, maintaining a general parallelism, for, at least, certain spaces. To these must be added, veins, which intersect all rocks; and those repositories of earthy and metallic minerals in a mixed or confused state, which occupy similar situations.

The unstratified rocks often form the highest ridges and summits of mountains, constituting the apparent bases on which the strata are placed; while, in other cases, under differences of constitution and in their periods of formation, they repose on the stratified rocks. In neither case therefore, are they limited to the higher grounds; as the positive altitude of rocks, or their relation to the surface of the earth, does not necessarily correspond with their geological altitude, or with that relative elevation which they possess in the series. For the same reason they do not invariably form mountains, although commonly occurring in mountainous countries. Lastly, they form but a small apparent part of the visible surface; whatever reasons we may have for believing that they occupy a great extent in the regions inaccessible to our sight or operations.

The great bulk of the accessible surface of the solid earth, is composed of stratified rocks, which, under different modes of distribution, form, not only the low plains, but the elevated mountains: being brought into view by their irregularities of position, and by that destruction which has so often laid them bare, and has generated the lower materials which, in other parts, conceal them from immediate examination.

To their variety of position is principally owing that inequality in the surface of the earth, by which it swells into hills or rises into mountains; although these forms have been, in a greater or less degree, influenced and modified by the actions which are daily operating on the surface; transferring the materials of the elevated grounds to the plains and valleys below, and burying many of them beneath the depths of the sea. Geology has distinguished these strata, according to their relative æras of formation, into primary and secondary: but each of these, and the latter in particular, involve subsidiary distinctions of great interest, which will form especial objects of discussion hereafter.

Although the forms of land are connected with and dependent on the dispositions of strata, as well as on those of the unstratified rocks, there is no rule, in this respect, universally applicable. A few examples will serve to convey that general notion of this subject which is all that is required for the present purpose. If a series of strata be perfectly horizontal, it is easy to understand that the land above it will have a corresponding level. If it be slightly elevated, it must, unless it be curved, unavoidably terminate somewhere, by fracture or discontinuity; and the surface will thus form a low ridge, of which the declivity which lies on the plane of the stratum, will be more gentle than that which belongs to the abrupt edges. The continuity of that ridge being interrupted by other transverse fractures, and other ridges occurring in a disordered manner, there is thus produced a land of low hills, which may also be equally generated by analogous irregularities in the forms and dispositions of unstratified rocks.

If such an elevation of a series of strata be increased, there is produced a mountain ridge, which may also be broken into detached points, so as to form irregular but continued chains. Here the planes of the strata will form declivities more or less smooth; while the abrupt edges will become the precipices so common in mountainous regions. Undulations of such strata, which have been here supposed straight, will generate corresponding irregularities; and thus, in low countries in particular, the inequalities are often produced, as much by undulation, as by unequal elevation and fracture; while they are also occasionally the result of inequalities of thickness in particular parts, or of deficiencies of certain portions of some stratum.

The case above stated, of a single uniform elevation, conveys the simplest idea of a mountain ridge of stratified rocks. But ridges are sometimes found, in which the direction of the stratification is oblique, not parallel. An example of this disposition occurs in our own country, in Bute, and may be observed in many other places. It is conspicuous, according to Humboldt, in the great chain of Mexico. Even in these cases however, where single ridges may not follow the direction of the strata, that direction maintains its general bearing; and it is a remarkable fact, although unwarrantably extended much further than it will bear, that in many parts of the world, and over very large spaces, the strata hold a parallel direction according to their elevated edges; and that if we take all that have been examined, there are more tracts in which that direction tends towards the North-east, than to any other point of the compass. This is the case in Britain; and it is particularly sensible in Scotland, where the heights of the elevations and the facility of access, render this circumstance easily examined.

But even where strata are thus prolonged on one line of elevation, the quantity of that elevation varies in different places; and thus numerous other irregularities are produced in the forms of mountain ridges. As, lastly, the elevation of strata may increase till they become vertical, other cases are produced, by which the forms of mountains become still further modified.

It next happens, that instead of strata being simply elevated in one continuous mass, they are raised by successive intervals, forming stairs, by which the forms of mountains are also modified in various ways. It is also observed that, on opposite sides, they have sometimes reverse inclinations; and thus a mountain formed of stratified rocks alone, may have similar opposing sides, unvaried by precipices or projecting rocks. Similar effects result from great undulations or curvatures of the strata; the effects thus produced in mountains, being hence of an analogous nature to those which take place in the lower grounds, but in a more extreme degree. These cases are, however, most frequent where the central parts of mountains or ridges are formed of unstratified rocks; against which the strata are sometimes found to repose, in opposite ways, and with different inclinations.

But all mountains are not necessarily generated, either by the protuberance of unstratified rocks or the irregular elevations of stratified ones. On the contrary, many high ones, of which Scotland offers examples on the west coast of Rossshire, are constituted of strata nearly horizontal; and their forms must then be attributed to that waste visible on their abrupt sides, in consequence of which all those portions have been removed, as far as their absence was necessary to the ultimate production of the shapes which they now display.

The formation of valleys, like that of hills, depends primarily on the elevations of the strata; and the one is a necessary consequence of the other. Nor is it possible to conceive that original cause wanting; unless it could be shown that, during the operations which displaced the strata from their original horizontal positions, every interval was so filled up as to form a smooth and level surface. Much has been written on a subject which appears too obvious to admit of any dispute; were we even to admit that the unstratified rocks, which every thing in their nature proves to have been originally irregular, had also once formed portions of an universal level. The original forms thus produced by the elevations of the strata and the protrusion of the unstratified rocks, have indeed been modified by the action of running waters, and often to a great extent; and thus only can rivers be said to have generated their present beds and the valleys through which they now flow. A very slender consideration of a state of things so obviously necessary, might have saved pages of useless, and often, of sufficiently acrimonious discussion.

In thus giving a general sketch of the distribution of strata, and of their different elevations and positions, as influencing the forms of land, it is yet necessary to remark, that these, hitherto distinguished into antient and recent, or primary and secondary, are, in some parts of the world, followed by a third set, which have been produced under peculiar circumstances. These, indeed, are distinctly divisible into two, arising from two distinct sources; the one having been formed after the secondary strata, in the basins of antient lakes and in æstuaries, and the others elevated from the bottom of the sea by the power of volcanoes, producing islands or portions of continents.

Volcanic products form the last, and are among the most recent of the rocks on the surface of the globe; though differing much in point of antiquity, and being, even now, formed under our eyes. These are, invariably, independent productions, although widely scattered over the surface of the earth.

Veins formed of rock, are either independent, or connected with the unstratified substances; and, however interesting from their origin and phenomena, they occupy but very small spaces, and have no effect in modifying the general surface. They are necessarily posterior to the rocks which they intersect; and thus a judgment can sometimes be formed, within certain limits, of the period at which they were produced.

The last of the hard or rocky materials of the earth, consists of those veins, also posterior to many of the rocks, which are the repositories of metals, as well as of many earthy minerals, and which, from their œconomical value, become objects of great importance.

The loose materials of the earth are found deposited over the surface; generally concealing the rocks from view, but distributed in a very unequal manner in different places. Where they merely cover the surfaces of the hills or plains, they only modify the forms of those; concealing asperities, filling valleys, or obliterating lakes. But they often constitute large portions of the surface, by themselves; limiting this remark, by geographical considerations, to the mean level as determined by the sea. This is peculiarly the case at the feet of high or extensive tracts of mountain land; and particularly where large rivers are found, which, depositing the ruins they carry along, cause the sea to retire, and form extensive plains. The consideration of this very interesting branch of Geology, belongs to the history of alluvia, and to that of the changes which the surface of the earth is daily undergoing; and it will hereafter form a distinct object of examination. It is unnecessary to extend further a sketch which is merely intended as an introduction to the succeeding parts of this work.

CHAP. VII.

On the Characters and Forms of Strata, and on Stratification.

As THE stratified rocks form the far larger portion of the visible solid materials of the earth, we are able the more easily to satisfy ourselves respecting their nature and relations, and to determine, by actual observation, much of that which, in the case of the unstratified substances, is matter of inference from limited facts. If there have been geologists unwilling to admit of the stratification of rocks, there have been others who have seen strata where they had no existence. It is the business of science coldly to investigate truth.

The term stratum, or bed, carries its own definition with it; its extent, according to the prolongation of its great opposing planes, being generally far greater than its thickness. A repetition of such beds forms a series of strata; and the term stratification implies the mode of their deposition, to whatever cause that may be attributed. Such masses, of analogous or similar shape, as are occasionally found among the rocks described in the tenth chapter, are however excluded from this definition. Their origin and forms are attributed, from the evidence elsewhere stated, to a different cause; and the mode of distinguishing them is also specified in its proper place. The term stratification therefore implies a cause, as well as a mode of form and disposition; and that cause is assumed, or proved, to consist in a deposition from water, of materials that have been suspended and dissolved in it.

Of the Forms of Strata.

The most perfect form of a stratum, is that in which the two planes are accurately parallel, but it is the most rare. They are more commonly inclined in different ways; so that a bed terminates at length, in one or more directions, or in all, by a thin edge; while it may also present surfaces so frequently and unequally inclined or undulated, as to be of various degrees of thickness throughout.

The thickness of a stratum may vary from one of many yards to that of paper; and it is obvious that the thinner cannot easily be very extensive. The extent of surface which any one may cover is equally various: it may amount to many miles; but, in these cases, it is traced rather by comparing detached parts, than by a continued view of the whole. That comparison is made by means of the consistent mineral nature of all the parts, the resemblance of the organic contents, where these are present, the correspondence and nature of the other strata with which they are in contact, and the similarity of position which they possess towards the perpendicular.

Thus it is anticipated that a stratum may be inclined to the horizon; but, in fact, they are rarely quite horizontal. The deviation from the horizontal position constitutes the inclination of a stratum; and the true inclination is evidently the greatest angle which a line taken on that plane forms with the perpendicular; the dip implying, further, that point of the compass towards which it is directed. As, in consequence of the inclination of a stratum, its edge must somewhere appear at the surface of the earth, an imaginary line has been contrived to represent it, called the direction of the stratum, drawn at right angles to the line of inclination. But the student must not imagine that the stratum has a greater extent on this line than on the reverse, or is of a long and narrow form: that can only be said of its visible portion; and a horizontal bed, having no dip, can have no direction.

Strata are subject to the various accidents of fracture, displacement, flexure, and contortion; and in some very rare cases, they even lose their regular forms and become shapeless masses.

The forms of strata are far more perfect in the more recent than in the antient series; and, in the latter, they are sometimes either so obscure or so difficult to discover, that their existence has been altogether denied. Hence, in some degree, has arisen a most pernicious confusion in geological descriptions and reasonings, which it has here been attempted to remedy by separating the unstratified rocks from them. The cause of this imperfection, in the older rocks, will be found to lie in the changes of their positions, and in the disturbance of their regularity, from flexure or fracture; in the less definite and frequent alternations of rocks of different characters which they present; and, among the argillaceous schists, from confounding the schistose structure with the planes of stratification.

Most commonly, a single bed consists of only one substance; but the materials occasionally differ in size in different parts, and that change occurs, either laterally, according to the plane of the stratum, or in the opposite direction, according to its depth. Changes of the absolute quality of the rock in a stratum, are far more rare. Many rocks are subject to be divided, by joints, at some angle to the plane of the stratification; and these are often remarkably regular, so as to separate the bed into cuboidal or other forms, of considerable accuracy. Thus, in the sandstones, there are often produced subsidiary forms resembling those of granite; and this, among other causes, has aided in supporting the erroneous belief in the stratification of that substance. In other cases, they confuse the inexperienced observer, by misleading him respecting the planes of stratification; as happens particularly in the argillaceous schists. In the Diallage rock of Shetland, the fissures are so numerous and extensive as entirely to suppress the appearances of the separating planes of the strata; so that, in determining the positions of these, recourse must be had to other indications, derived principally from the more regular adjoining rocks.

When, by the repetition of beds, either of the same or of different substances, there are formed successions or series of strata, the knowledge which we obtain of the various rocks, is derived chiefly from that obliquity of position which is fully discussed in the next chapter; in consequence of which, united to its various accidents, every member of a considerable series is sometimes brought into view, even within a narrow space. By a careful comparison of positions, the same object is effected, through many interruptions, and over districts of great extent.

It is sometimes observed that such a series preserves, for a great space, a very accurate general parallelism; the different beds being parallel among themselves. But, very frequently, the whole series is bounded, as a single bed may be, by inclined planes; in consequence of which it gradually vanishes by extenuation. The other accidents of a single stratum are also true of a whole series; which may thus vary, by undulation or flexure, either in the line of its inclination, or in that of its direction. Thus it becomes necessary that observations on these lines should be regulated by a general average, and not by measurements taken in a single place, which are a frequent source of error.

The disappearance of strata which is not the result of extenuation, may arise from their dipping out of sight in a gradual manner, or from those fractures or subsidences by which they are entirely broken off. This fracture is, of course, found at the elevated edge; but the strata are sometimes renewed in a new position near it, in consequence of the subsidence.

In a series of beds, the same substances are sometimes repeated in succession; at others, beds of different natures succeed in a certain order, or in repeated alternations, more or less numerous or regular. Where the beds are similar, they are sometimes separated by mere planes, resembling fissures; but it is more common to find some slight change of texture, or some intervening material, however small in quantity, indicating the place of separation. When the beds are of different kinds, the mode of separation is generally very distinct; and, in some cases, in the secondary, it is effected by the accumulation of animal or vegetable remains in a thin lamina. The laminar disposition of the beds themselves must be considered as, in itself a result of stratification; and I have elsewhere remarked that it must not be confounded with the concretionary structure. (Chap. xi.)

Of the Positions and Relations of Strata.

Consecutive and parallel order is a natural consequence of the process of stratification. But this is confined within certain limits, as noticed in the next Chapter on the dispositions of strata. Near the unstratified rocks also, that order ceases; or any number of the strata in one series, may be in contact with a rock of this character; a natural consequence resulting from the mode in which these rocks were formed, and often very conspicuous at the junctions of the primary strata with granite.

There is another case in which strata cease to be consecutive, without being in all parts unconformable to those on which they lie. In the usual reversal of position, the upper series may be conceived to lie on the elevated edges of the inferior one; although, even in such cases as this, one part of the upper series may be conformable, while the other is reversed, in consequence of irregularities in the position of the inferior strata, or from the superior being so deposited as, in some places, to lie on the edges, and, in others, on the planes of the inferior beds. This is easily understood. In the case above alluded to, an effect of a contrary kind takes place; or every stratum in an upper series, touches a single bed of the lower; the edges of the former abutting against the planes of the latter. It is easy to comprehend how this may happen. It is a necessary consequence of the deposition of a horizontal series upon an inclined one, and occurs, as a matter of course, in the recent depositions of lakes that are contained in cavities among the usual primary or secondary strata which are so rarely horizontal. That is a simple and intelligible case; and it is here also easy to see, that, in another part of such a deposit, a very mixed and uncertain collision must take place among the edges of the superior and the inferior strata. In nature, this happens on a much greater scale, and is often a source of difficulty to geologists; to whom, if improperly viewed or reasoned on, it presents appearances of disturbance which have never existed. It is a necessary result in those extensive deposits which must be considered of a partial nature, and which have been called basins; and the coal series, in most situations, presents conspicuous examples of it, which have often been much misapprehended.

It is a general remark, that on opposite sides of a hill or of a ridge, even though it should be a mountain of granite, the same series of strata will occur in a corresponding order: deposited on it in two places by one general law, according to some geologists, but being one series separated by its interposition, according to the views entertained in this work. Yet, in some situations, the strata on opposite sides of a ridge differ; and this may be explained by considering them as separate deposits that have been formed from different original sources in different cavities or basins.

This fact, under various modifications, is of very common occurrence, and contradicts the doctrine of universal formations; the imaginary nature of which is noticed when treating of the order of succession among strata. (Chap. xiv.) The wish to extend analogies is perhaps natural to the human mind; and, in this case among others, it has had an unfortunate effect in Geology. It thus becomes the object of the observer, not so much to investigate accurately and describe carefully what is before him, as to decide whether a stratum belongs to this or the other of some series which he has made his standard of comparison. Thus even the accurate English geologist may fall under the reproach which has been peculiarly bestowed on Werner; finding a Britain whereever the latter discovered a Saxony. "Affingit parallela et correspondentia et relativa quæ non sunt," is the censure which Bacon has applied to philosophers who thus exceed the limits of that which can be useful only when kept under due restraint.

Whatever analogies therefore may be found all over the world, not only among the natures of the strata, but in their relative order of stratification, there is no where that resemblance which can authorize us in supposing that they have either been simultaneous or under the influence of an universal law. The instances hereafter quoted in the Chapter on the actual successions of rocks, will illustrate and confirm this view. But the resemblances are more general, or rather the analogy is more extensive, among the most ancient strata than among the more recent; and as we ascend in the order of the series, or descend in the order of time, the discrepancies increase. This is what might be expected, even from contemplating the view elsewhere given of the composition of rocks; the more ancient appearing to have been at one time under the influence of a common power from which the others have been exempt. Thus we may perhaps account for the prevalence of gneiss, hornblende schist, and micaceous schist, among the lowest strata; although the nature of the rocks whence the original materials were procured, may also have been a modifying cause. But even here, it will hereafter be shown, that the order of succession is only general, and very far indeed from being so particular as it has been imagined.

The further we examine upwards, the more we shall be convinced that all successions are analogous, and not identical; though the accuracy of the analogy in the order of succession, or the extent covered by some one definite series, is extremely various. By commencing at the very surface of the earth, we shall more easily comprehend the truth of this remark, and the probable cause in which these differences have originated.

The smallest deposits which we know, are those formed in lakes and æstuaries; and if we examine these, although we shall find that they all contain alternations or successions of mud, shells, clay, and sand, yet we shall see that there are probably no two in which these materials are deposited in the same order or proportions. Here is a perfect analogy, but no identity; and we can assign the reasons with the greatest confidence, from knowing the various nature of the substances deposited in them by the rivers, and the unequal fertility of the organic beings which they have nourished. In these deposits we find a fair analogy, because in them we trace the germs of the rocky strata.

The uppermost of those deposits which are sufficiently antient to have assumed the character of rock, are those known by the name of fresh water formations; and these, it is probable, have been deposited in the basins of antient lakes and æstuaries. Among those that have hitherto been carefully examined, we also trace similar analogies, but no identities.

It is at a considerable distance from these, downwards in the order of the strata, that we find another and a very important class of deposits, namely, the coal strata. In these also there is a general analogy throughout; but, in no two do the substances occur exactly in the same proportion or order; while we can here also assign the probable cause, by knowing that they are deposits from fresh water, and therefore probably formed in separate lakes.

It is unnecessary to prolong this reasoning, as its object must already be obvious. Whatever seas received the materials that have formed the different secondary strata, it is plain that the furthest limit of identity among these strata thus deposited beneath it, will be bounded by the extent of any one; while, in different seas, we must expect different, although analogous, collections of strata. It is not however neccessary that, even in one submarine cavity, or within the limits of one sea, the strata should be every where identical. Different rivers may have entered by different æstuaries, and the depositions from these must have varied in character and manner, according to the materials which they deposited, or according to the nature of the mountains in which their sources lay. This supposition is confirmed by examining the English series, of which an account is given in a future chapter. There is a certain general order in that part of it which lies about the coal strata, such as may make us conclude that it was deposited in one period of repose and in one cavity; but if we examine the proportions and positions of the different members throughout the whole space, there are differences which bespeak analogous variations in the deposited materials.

This is confirmed in another way, by examining those submarine depositions, now within our reach, which are probably destined to form future strata, and which are, to all appearance, the copies of those from which the consolidated strata have originated. It is so well known to mariners that the alluvia which cover the bottom of the English Channel differ in different places, that these variations are used as a guide in navigating that sea. It is by the quality of the soundings, as these may consist of sand, of mud, of shells of particular character, or of various mixtures of those, that the pilot determines his place when the darkness of night or the haziness of the weather conceals the land from his view.

It has been observed that, in many places, certain strata of different natures are usually associated in groups, as they may be termed, or that one rock of a remarkable character is interstratified with a minor quantity of others, for a certain depth before it finally ceases to give way to another equally conspicuous. Thus, for example, gneiss and hornblende schist are frequently found united, and the latter in small proportion to the former; and thus the red marl sandstone of England, contains beds of clay, shale, and limestone. These minor strata have been called subordinate; a term, the nature and abuse of which is examined in another place; and to these associations the very awkward name *formation* has been applied. It has been supposed that the invention of this term has been of great use to Geology, by generalizing certain observations; and it is also imagined that many such *formations* are really definite and constant. That cannot be denied; nor will the utility of the expedient, in some cases, be questioned. But it is obvious that this scheme savours too much of hypothesis, and that it may become a fertile source of that gratuitous and unfounded generalization against which the young geologist has already been cautioned.

Of the Nature and Causes of Stratification.

That the strata could not have been deposited from solution in water, is sufficiently shown elsewhere, and it is therefore unnecessary to dwell on it here (Chap. xii.) It is to mechanical deposition, in a more or less gradual manner, and from suspension in water, that we must attribute their origin, as is more fully shown in the same chapter. If any thing could prove the gradual and mechanical nature of this process, it is the state and existence of fossil shells in the calcareous strata, the successions of generations of animals entangled in mud, arising partly from their own sources, partly from extraneous materials, and deposited according to the strict rules of mechanical arrangement. The Oyster banks of our own times are their copies or types; and they wait only for that time and these changes, by aid of which they are destined to form the limestones of a future earth.

The preparation of the strata is a tedious process: that it has been the work of incalculable time, requires no other proof than that now mentioned; though endless proofs of the same nature are found in the nature of the strata themselves, and in the changes now going on upon the surface of the earth.

The deposition of regular strata of loose materials in distinct succession, may be seen every day, in the sections of lakes and marine æstuaries. A lake is but the image and model of a sea; an æstuary is a portion of the ocean itself. By whatever hydrostatic or mechanical laws, therefore, the process of stratification has been regulated in these, it must have been similarly regulated in the ocean itself. It is a problem in which magnitude does not form an element of calculation. As far as relates to a single deposit, even the precise mode in which the materials are distributed on the bottom can be defined; since it consists in a combination of the force of gravity and the resistance and motion of water. The diminution of the former power, arising from the different specific gravities of the earths and of water, added to the minuteness of the materials, which thus expose a large relative surface to the mechanical impulse of the water, permits them to be floated beneath it to great distances, by a succession of minute actions, till they finally settle immoveably on the bottom. This very process may be witnessed at the mouths of great æstuaries, as well as in lakes. In the latter, as in the former, the motions of the rivers first carry the materials forward to certain distances proportioned to their power; and, in the former, this action is incessantly renewed by the daily motions of the tides; in both, by the restless state of the water. The distance to which mud is carried into the ocean by the great rivers of America, is very great; and, to a less degree, the same phenomena are visible on all sea coasts; while the renewal or change of place, in deposits of mud or sand, by the action of the tides and by gales of wind, is familiar to every mariner.

But this operation is also carried on in those depths which our eyes cannot reach; as is proved by the sounding-line. Were the bottom of the sea not covered with loose materials, soundings would every where be clean and rocky; whereas mariners well know that this is a very rare occurrence, and that it exists only where elevated rocks are found; forms on which mud could not settle. That this state of the bottom occurs all over the ocean, we cannot prove; as the depths of soundings are limited, and, owing to the negligence of seamen, have been held far more so than they actually are. But no soundings have ever yet been made where mud and loose matters have not been found, although at hundreds of miles from the nearest land; and in the trials, in Baffin's Bay, fragments of limestone with calcareous mud were brought up from depths of a thousand fathoms.

Not only the deposition of loose materials, but the separation of different kinds, is produced by water. In lakes and æstuaries, this is witnessed in the deposits themselves, which present successions of mud, of sand, and of gravel. There is no difficulty in understanding the nature of this process; since it is the necessary consequence of the comparative sizes of the materials, or, more correctly speaking, of the differences of ratio between the contents and the surfaces of the respective solids. The regular interstratification of sand and mica on sea shores, is a familiar example of this fact.

Thus we provide, without difficulty, for one succession at least, of sand or gravel, and clay, the germs of future sandstone and shale. By the assistance of marine animals, whose colonies are distributed on the bottom, we also obtain a calcareous stratum; and we are thus furnished, with the essence, at least, of every stratum which nature has produced among the secondary ones. Such of the causes of their variations as we have been able to conjecture, are stated in their proper place, as are those by which the primary and secondary are distinguished. Whence those very frequent alternations have arisen among them, geologists have not yet been able to discover; but the general principle is established, when it is shown that a single series of three can be produced, and is actually formed, by these operations. It must indeed be apparent, that in a series of revolutions, probably of a very intricate nature, and through an incalculable lapse of time during which the materials must have often been changed, it is impossible to discover all the circumstances by which these alternations have been regulated. But it must still be remembered, that as far as transportation and deposition alone are concerned, we are bound to explain only the alternations of sandstone and shale, or clay, in which there can be no great difficulty. Those of limestones have been regulated by the casual establishment of animal colonies in beds favourable to their existence and reproduction; as it is elsewhere shown that the calcareous secondary strata are chiefly of animal origin. But indeed we need not rest the decision of this question on grounds merely possible; since, in the phenomena of lakes and æstuaries, we have convincing proof of alternations, not only of clay and sand, produced from the materials brought down by rivers, but of beds of shells and calcareous mud also.

Thus the origin of strata is derived from depositions of the materials of the dry land under the waters of the sea, and, in some cases, of great inland lakes, intermixed with the spoils of animals that have lived and died through a long succession of ages. If the daily causes of waste pulverize the solid mountains, and the rivers transport their ruins to the sea, so, other causes, acting more extensively and powerfully, must be allowed a share in producing and depositing the materials to which we owe our present stratified rocks. The extent and nature of these operations will be fully examined in its proper place, as they are now in progress, or are past, and as they include that interesting branch of geology which relates to the present surface of the earth. (Chap. xxii.) In the ruins of an antient earth we find the materials which form the present; as, in the destruction of the land which we now inhabit, nature seems to be preparing habitations for future races of animated beings.

But though I have here said, that causes operating more extensively and powerfully than the slow actions of waste and transportation, may have aided in preparing the materials of the strata, we must beware of allowing more effect to these than they were capable of producing; as has been done by those who object to certain geological claims on indefinite Time, and who seek for solutions in transitory diluvian powers. The effects of such torrents must have been to deposit mixed materials of various sizes, in a confused manner; and they could therefore have prepared the germs of the conglomerate strata only. The strata formed of finer materials must have been the consequences of tedious actions, analogous to those which we daily witness; while their separation into distinct rocks, into alternations of clay and sand producing schist and sandstone, must have equally been the work of a slow process beneath the water. And thus also we discover, from the comparatively small proportion of the mixed and conglomerate strata, the small ratio which, in antient states of the globe, these tumultuary actions have borne to the more gradual ones; while, from the concurrence of these strata, in place, with other appearances indicating revolutions, we are enabled to conjecture the causes which produced these diluvian actions.

It has been remarked that consolidated and unconsolidated strata alternate, and that strata are not deposited in the order of their specific gravity. Both of these facts have been urged against certain geological theories; but they will probably admit of explanation without much difficulty.

Though the consolidation of strata is not here attributed to heat, the alternation of unconsolidated clay or sand with solid rocks proves nothing against such a supposition. The heated, or rather the fluid original state of trap veins is admitted; and it is not unusual to find these in the form of pseudo-strata, or parallel veins, alternating with beds of clay; while masses of trap are also found incumbent on similar beds. In such cases, the clay is often converted into jasper, in others it is partially changed, while, in a third case it has escaped entirely. Sky presents the most perfect display of all these phenomena. Thus, under minor degrees of heat, alternating beds might have escaped its action, without contravening the general principle, should any geologists be inclined to maintain it. But there is still another well-known fact which might be adduced in proof of the possibility of an alternation between consolidated and unconsolidated strata, even if heat were the indurating cause. This is the decomposition which rocks undergo, though deeply situated beneath the surface; a fact common in granite and gneiss, and even in trap to a great extent. It is as easy to imagine that rocks whose constitution admitted of it, should have undergone this change, since their consolidation, while the adjacent ones have escaped, as that, under the circumstances already stated, they should have escaped consolidation altogether.

The fact, of the uncertain recurrence of different specific gravities among the strata, is clearly no obstacle to the theory of stratification here laid down. That objection, commonly made, is founded on an equal ignorance of the chemistry of rocks and of mathematical principles; even were it admitted that all the strata had been deposited from a single mixture of earth and water. The subsidence of solids through water is regulated by other principles, as I have already shown, than that of mere specific gravity. But whatever the specific gravity of the materials may have been, that of the ultimate solid is partly regulated by its final state, namely the density consequent on its consolidation.

Thus, in a single series of two strata, the heaviest body might be uppermost; and, in fact, if quartz and ferruginous clay were suffered to subside together in water under agitation, that would be the case. As, in the present view of stratification, these subsidences have been gradual, and the consequence of distant and distinct operations, the whole objection falls to the ground.

It is as little an objection to the theory of successive stratification, that crystalline and earthy rocks alternate; since, under the innumerable variety of circumstances in which they have been formed, or to which they may have been exposed, and under the great variety of composition which they present, it would be much more surprising were this not the case. I need dwell on this supposed difficulty no longer; as the Geologist, who is inclined to examine the various cases that may occur, will find a guide to assist him in the chapter which treats of the consolidation of rocks.

The mixtures, gradations, and alternations, which take place between strata of different natures where they meet, have given rise to much dispute and speculation; but they do not appear difficult of explanation, on the simple principles which are indicated, here, and in various parts of this work. The explanations of individual cases must however depend on a variety of minute and local particulars, of which the explanation would be tedious, or, for want of minute and local knowledge of events long past, difficult.

Where clay and sand, to reassume a fundamental and easy case, are subsiding from water, it is obvious that there might be a race run, or a contest for time, between the most buoyant parts of the inferior, and the weightier of the superior deposit; whence would follow an intermixture of character. The same might occur from the mere action of water in motion, on the mutual confines of two substances yet free to move. As to that interstratification which produces beds properly called subordinate, it is an obvious consequence of more than one cause acting where a river or a diluvian torrent has wasted or transported different rocks, either at different periods, or from changes of place; as well as of mixed and varying hydrostatic actions which require only to be named. As to the variations in the characters of those strata which, like most or all of the primary, seem to have been subjected to the action of heat, it can require no further explanation than the general ones elsewhere given, of the actions of the unstratified rocks on the strata, and of the consolidation of rocks.

CHAP. VIII.

On the Dispositions, Fractures, and dislocations, of Strata.

WHEN we find the stratified rocks forming the summits of the highest mountains, elevated many thousands of feet above the level of the sea, and when we suppose that the objects which we are contemplating were once covered by a fluid, we are strongly impressed with the changes which the relative levels of the water and the land must have undergone, with the revolutions which the surface of the earth has experienced. And when we find the remains of shell-fish imbedded in these strata, we cannot hesitate to admit that these rocks have once been covered by the Ocean. When lastly we observe that those beds which must once have been horizontal are now vertical, that they are inclined, broken, bent, and dislocated, in innumerable ways, we are forcibly led to conclude that their present distance from the sea has been accompanied by violent alterations in the form of the surface, and that it has been produced by the action of enormous powers. An inquiry into the probable nature and causes of all these changes, will form a proper conclusion to a description of each of the various phenomena which the facts themselves present.

The horizontal position of strata is not incompatible with their situation on the summits of the highest mountains; neither are the inclined, nor even the vertical strata, excluded from the lowest grounds.

The sandstone mountains of Rossshire, formed of a strata nearly horizontal, exceed 3000 feet in altitude; 8.44
and, on the low southern shores of England, the chalk 8. II 214
beds are found in a vertical position; but it is, nevertheless, more common to find that mountainous countries are characterized by an inclined stratification. In some places, these strata are found to occupy the vertical angle; and, from that, they present every possible inclination down to the horizontal position. In a series of such inclined strata, presenting a conformity of inclination, the parallelism is not always perfect. It is not uncommon, on the contrary, to find that the angle of inclination gradually increases, or diminishes, according to the line on which the series is examined.

This is the simplest appearance of elevated strata: it is necessary to inquire into the causes which have been assigned for it; that the fundamental argument may not be unnecessarily incumbered with provisions for effects of a more complicated nature. The first question is, whether that distance between the sea and this its produce, is to be accounted for by the desertion of the latter, that is, by its subsidence to a lower level, or by the elevation of the land above the waters. It is unnecessary here to clear this question of the incumbrance it once suffered from those opinions which attributed the deposition of shells, and even of strata at these high elevations, to the imagined operations of the Mosaic deluge. The time of that reasoning, which equally despised philosophy and perverted Scripture, is past.

The subsidence of the ocean bears, on a first view, that appearance of facility which invariably misleads unreflecting observers and feeble reasoners. It has been ascertained by Ulloa, that the strata near Guancavelica in Peru, contain shells at an elevation of more than 14,000 feet above the level of the sea. To depress the ocean from that point to its present level, is to annihilate, or otherwise dispose of, all the water that would be required to fill the imaginary space between the spheroid whose diameter is bounded by the present level of the sea, and that which exceeds it by twice 14,000 feet. This is a problem for which neither chemistry nor geology has made provision. Nothing in nature is annihilated; nor will the laws of astronomy here concede that which those of chemistry alike refuse, when it is recollected that the imaginary mass of water in question, exceeds one five hundredth part of the bulk of the whole earth. To attempt to prove that it has not retreated within imagined abysses of the globe, would now be to argue, it is hoped, without opponents.

Even if all this could be admitted, and every advantage allowed, it will immediately appear that this supposition offers only a very partial explanation of the phenomena that attend elevated strata. An hypothesis that explains but one out of many concatenated effects, carries with it its own condemnation.

Let us now examine, on the other hand, the arguments which prove that the land has, on the contrary, quitted the sea; that it has been raised, by whatever causes, to its present elevation above the general level of the simple spheroid.

If the highly inclined position of strata were not in itself a proof of their elevation, evidences of motion are found in a great number of phenomena, which are more particularly described in their proper places. In their curvatures we find proofs of disturbance; we find even more decided evidence to the same purpose, in their fractures. But when we see that these fractures are often accompanied by a separation of the parts which were once continuous, that one portion of a stratum occupies a higher or lower place than another, and that this separation is often attended by a difference in the angle of inclination of the separated parts, we have every proof that can be desired, of an alteration in the positions of stratified rocks since the period at which they were formed or consolidated. Here we may safely reason from a part to the whole; from a single member in a connected train of phenomena, to all those by which it is accompanied.

In the nature of the materials, of which many of the inclined strata are formed, we find further proofs of their elevation. It is elsewhere shown, that many of them are composed of substances once separate; of sand, and of fragments of more antient rocks, consolidated by causes, respecting which it has been inquired in its proper place. (Chap. xii.) It is admitted that the finer materials, at least, need not necessarily have been deposited on a horizontal plane; but that they might rest beneath water at angles of considerable inclination, waiting for the period of their consolidation. But this admission is far too limited to be of use in explaining the cases that occur. It is notorious that the conglomerate rocks, which form such conspicuous strata in many countries, and which prevail chiefly at the boundary which separates the strata called secondary from the primary, are often found in positions, not only highly inclined, but absolutely vertical. As the materials of these are often of such bulk as to weigh, even many hundred pounds, it is evident that the original position of the strata which contain them, must have been horizontal.

It is well known that certain marine worms which live in sand and inhabit straight tubular shells, invariably penetrate the sand in a vertical direction. Analogous fossils found in the horizontal strata, preserve the same vertical position. But where such strata are inclined, the position of the animal is no longer vertical to the horizon, although it preserves its perpendicular position to the stratum; indicating the change of place which that has undergone since it formed a part of the ocean's bed. Further, when a concave shell subsides in water, it must necessarily rest with its convexity downwards; and thus the shells of such animals of this form as have died on submarine banks, are found in the mud, accumulated in this position. But in the elevated strata of rock in which such remains abound, they are found no longer to regard the horizontal plane; while their position, on the contrary, respects those of the strata; offering a test, in cases of great disturbance, by which the originally inferior part of any such stratum may be determined. The same conclusion follows from considering the positions of leaves in the strata that accompany coal. In these cases, the flat side of the leaf is invariably parallel to the plane of the stratum, although it may often be vertical to the horizon; a position, it is sufficiently plain, in which such substances could not have been deposited from water.

The general elevation of strata from the bottom of the ocean being thus proved, it remains to inquire respecting those varieties of appearance and of relative position, which are either interesting in themselves, or in their consequences, or which require for their explanation the aid of other accessary causes.

As strata may be elevated to the perpendicular position by causes or forces the exact nature of which we cannot ascertain, it is easy to imagine that they may pass the vertical line, or become to a certain degree reversed. It is very plain that, in such a case, errors might be easily committed respecting the relations of a particular series. If, for example, any portion of a simple series were thus to be reversed, so that the one part should approach towards a parallel direction with the other, an unreal alternation would appear to exist; or that stratum which was in one place the inferior, would, in another, become the uppermost. This is a case, however, which, must always be difficult to prove; although Saussure and others have supposed that they have ascertained examples of it. It is evidently, not only a possible but a probable occurrence, when we consider the extreme motions which strata have often undergone; but to determine it truly where it exists, must be left to the sagacity and knowledge of the geologist; as no rules could be given but those which every one may deduce for himself when acquainted with the phenomena of rocks in nature.

Contrariety of position forms one of the most interesting and important circumstances attending on the changes which strata have undergone; not only offering proofs of partial and distinct æras of disturbance, but presenting some of the most satisfactory evidence that exists, respecting the successive changes which appear in the constitution of the globe.

The simplest case of this, perhaps, is that in which approximate strata are merely inclined to each other in opposing directions, and where no one is distinctly placed towards the next in a reverse or unconformable position. A simple series of this nature, consists of beds inclined to each other in such a manner as to respect one vertical line. These may converge from an imaginary plane, upwards to the perpendicular, or from a similar plane downwards; in which latter case they have been called divergent strata; in both, the term radiated has been applied to them.

Such a series is subject to change its inclination in the course of its progress from the imaginary perpendicular; the angles gradually diminishing till they become horizontal; and, to this modification, the term of radiated ought in strictness to be limited. It is further not uncommon to find a series of this nature exceedingly complicated; the angles of convergence and divergence occurring alternately, together with intermediate horizontal portions. The western coast of Argyllshire offers a very interesting and extensive example, in a series containing Chlorite schist and other rocks, which is described in another part of this work. (Chap. xxvii.)

The appearances under review are interesting, whether we advert to their probable causes, or to the condition of the strata before these changes took place. It must also be evident, that they tend to confound all our reasonings respecting the actual or original alternations of the strata; as it is scarcely possible, in complicated cases, to determine what belongs to original position and what to disturbance; how far the succession is simple, or how far it has been multiplied by the repetitions consequent on change of place.

To assume a simple case, it is evident that a series of strata becomes doubled by one convergence; and, if these be multiplied, there is no limit to the apparent repetitions that may ensue. Thus, for example, a series of four rocks, alternately converging to points above and below the surface, so as to form six complete angles, will present the deceptive appearance of twenty-eight alternations; while it is further evident, that the true superposition of the original strata is entirely perverted. The results of these effects correspond to those hereafter described in the case of undulations, where the upper points of flexure have been removed.

That this statement is not visionary, is proved by numerous examples on a small scale; where not only the fact itself has been clearly ascertained by natural sections, but rendered evident by the peculiar nature of the strata. That it may exist on a very extensive scale also, is rendered highly probable by the facts already noticed respecting the series of Argyllshire; a tract more fully described in the account of the Western Isles. The whole of this tract may be considered as exceeding twenty miles, on a line taken transversely to the bearings of the strata; and throughout this space, computing from enumerations taken at different places, there are probably not less than 40,000 strata. Yet the rocks of which they all consist are limited essentially to four, namely, quartz rock, micaceous schist, chlorite schist, and hornblende schist; the minor variations being of comparatively little moment, and easily conceived to be mere varieties of these four fundamental substances. Throughout this whole space, these strata present an incalculable number of discordant positions, consisting in that frequent alternation of opposite inclinations already described, intermixed with horizontal and vertical beds. It is probably therefore, not unreasonable to suppose, that this tract consisted once of a series of horizontal strata, of perhaps four substances only; and that, in consequence of numerous displacements, they have assumed the complicated and deceptive appearance which they now present.

That such a series would occupy a less horizontal space after disturbance than before it, is a difficulty that occurs to a greater or less degree in almost every instance of similar elevations; but it is not such as to alter our general conclusions respecting the real nature of these phenomena.

As the general causes of the elevations of strata will necessarily be considered hereafter, it is only now requisite to say, that, whatever these may be, it is easy to apply them to the production of the most complicated of these effects, by merely varying their energy or multiplying their places, or repetitions. It can hardly be necessary to point out, that one elevation at a given point would produce a simple series converging above the surface, and that two would give rise to a convergence downwards in the intermediate space. If subsidence be resorted to for the explanation, it is only requisite to reverse this statement; and it is further evident, that the most extensive and discordant series that can exist, will admit of a similar mode of explanation.

Of the Depth of Strata beneath the Surface.

It has been ignorantly made a matter of reproach to geologists, that they reasoned respecting the structure of an earth, to which they had no further access than by operations that ought to be considered but as scratches on its surface. It has been said that the highest mountains were but as dust, and the deepest mines but as invisible punctures on a common geographical globe. But a consideration of the elevated positions of strata, and of the consequences which may thence be inferred, are sufficient to show that this reproach is as unfounded in fact, as it is bottomed on an ignorance of geological phenomena.

If we were to assume, in the simple case of a vertical stratum, that it had been formed in this position, we have no reason for denying, that for aught we can prove to the contrary, it may reach to the centre of the globe, thus rendering our knowledge of the deepest regions as perfect as that of the surface. If, on the contrary, we suppose that such a stratum has once been horizontal, and has become vertical from elevation, we have only to measure the superficial extent of any large similar stratum or series of strata which has been observed, and, by placing it in the same position, we can infer the probable depth below the surface, from that which is visible above it.

But to proceed more accurately, let us suppose a great horizontal tract where the edges of strata are exposed. If we measure these at right angles to the plane of stratification, and then suppose them restored to their original horizontal position, we have discovered the structure of the earth to at least the same depth.

This is not an imaginary case, though put in an imaginary form; since examples of it occur all over the world. The erected edges of a series of strata are often seen following each other over large horizontal spaces; they are often found in the acuter rocky declivities of mountains, or in vertical cliffs. By a simple trigonometrical operation, it is easy to compute the thickness of a series; or it might be measured by a repeated addition of the dimensions of the integrant strata. That thickness is the measure of the original depth before elevation. Thus also it may be computed to what depth from the surface, at any given spot, it would be necessary to sink before reaching the lowest bed; which is evidently the very depth, be it what it may, that is actually exposed to the light of day. It may be much more; and thus we see within the earth as perfectly as if it were transparent. It is unnecessary to name examples of a fact which occurs in every part of the world; but it has been computed by Mr. Playfair from an observation of Pallas, that in this one instance quoted from him, we probably gain access to 61 miles in depth, or nearly a sixty-fifth part of the radius of the globe.

It is however necessary to modify this general conclusion. The unterminable, or even distant prolongation of inclined strata downwards, is no more probable than their uninterrupted continuity on the surface. All rocks appear to be, in a certain sense, partial and limited. They are, moreover, subject to fractures and displacements, which must interfere with such conclusions respecting their prolongations beneath the surface. The very fact of elevation and displacement implies this; while we have, otherwise, no reasons, *a priori*, for supposing that strata deposited from an ocean, however antient, can exist beyond a certain, however undefined, depth; while the repetitions of beds in contrary positions, assure us that the depositions of such strata have been the consequences of alternate destruction and renewal.

An example or two will serve to illustrate the nature of the limitations to be made in reasoning respecting the depths of strata beneath the surface. In the Argyllshire series already described, it was shown that the appearances were easily accounted for by a theory which would prove their depth, either now or in their original position, to be very limited. Nor is there any reason to imagine that they extend to great depths; on the contrary, the very nature and multiplicity of their disturbances, argue, not only for their own limited extent, but show how similar causes may have equally limited the subterrene extent of strata in other places, where, either the appearances are not so obvious, or have not been so perfectly investigated.

It is not unusual to meet with a long succession of alternating strata, following each other with a common, though variable inclination. According to the rules above laid down for computing the original depth of such strata, or the access which we thus gain within the earth, we might conclude that we had penetrated far beneath it. Yet we might here be easily deceived. If it be imagined that a horizontal series was elevated at different points in succession, so as to preserve such a consecutive disposition as may aptly be represented by the teeth of a saw, it is plain that a very small number of original strata would give rise to a great series of inclined ones, and that we might form a very false conclusion respecting the real nature of such a series. It is probable, from some appearances occurring on the west coast of Scotland and among the neighbouring islands, that this is actually the case in that tract; and we are thus also easily led to explain that repeated succession of a very limited number of strata in consecutive order, which is found among the islands in question.

Of unconformable Strata.

The next case of contrariety in the relative positions of strata, is of much greater interest; as affording evidence of some of the most important changes which have occurred on the surface of the earth. It consists in that difference of position between approximate strata which has been called unconformable. In this modification, the strata not only depart from a general parallel order, but the change is so abruptly made, that one series, or stratum, is found placed at angles to another, and in contact; so as that the planes of the superior repose on the edges of the inferior. The angle at which this species of junction may take place, is variable; and its variations lead to no conclusions that are not very obvious.

It is easy to understand the nature of this appearance. If we examine the deserted bed of a lake, in a stratified country of the usual elevated structure, we shall find horizontal strata of matters more or less indurated, deposited on the rocks beneath, in reverse order. Here then is a combination of events, of which we see one half, and infer, with tolerable certainty, the other. The conclusion is natural; that a period has elapsed between the production of these two orders of strata, and that, during this interval, the inferior series had been displaced from their original position. Extending this reasoning to the uppermost class of rocky strata with which we are acquainted, namely, the fresh water or tertiary deposits, we assign an interval of time, in the same manner, to the difference of position which separates them from the secondary or marine strata beneath. And thus again, in the same way, we discover another revolution and another interval, at the boundary which separates the secondary from the primary strata; indicated equally by reverse order of position. It is not the place here to pursue the train of reasoning to which these appearances lead, nor to specify all the instances in which these reversals of position occur. A more proper place for that discussion will be found hereafter. (Chap xxi.) It is sufficient to remark, that they prove a series of changes, with intermediate intervals of repose; changes which have elevated in succession the strata that had been once deposited in a horizontal position, and times of rest permitting the formation of new deposits.

It is evident, that the variations in the appearances, at these junctions of unconformable strata, may be considerable; as there is no limit to the varieties of position which the inferior strata may have assumed. The angles at which they meet may amount to a right angle: they may be infinitely small, or parallel, even between those rocks which, at another point, may be completely reversed; since these circumstances must depend on the discordant or regular position of the fundamental strata. It is plain, for example, that a horizontal series of strata now deposited on the Argyllshire series already described, would occupy every possible relative position to the same rock. Thus, although a reverse position is always sufficient to prove an interval of time between successive strata, its absence is not always evidence of the negative.

The simplest case only has been stated; namely, that in which the superior strata are horizontal, whatever may be the position of the inferior. But the superior strata are often highly inclined, while the inferior are still unconformable to them. It is possible that, in such a case, the most antient strata may be the least inclined of the two: they may even be horizontal. Whatever may be the fact in this respect, it is plain that the inclined state of the superior strata proves that even they have been displaced since their deposition; and thus it is also proved that the inferior strata have been twice displaced; once to assume the inclined position themselves, and a second time to communicate it to those of which they formed the basis and support. Other proofs of two displacements of the inferior strata, will be found in the phenomena of fractures and shifts hereafter described.

As it has been shown that there have been successive displacements and depositions, it is next easy to see that three or more reversals of position might occur in a long series, in which all the parts were open to investigation. This indeed may be inferred without difficulty, from the comparison of the positions of different sets of strata; but I cannot quote any instance in which more than two have actually been observed in one spot. But it is unnecessary to dwell on this subject. Sufficient has been said to show, that in the proofs which we have of successive changes in the positions of strata, provision is made for explaining all the confusion which is manifest in the disposition of the rocks at the surface.

Of Fractures of the Strata.

If strata had been deposited where they are now found, there could be no reason for expecting any breaches of their continuity. Such accidents are nevertheless common, and they serve, not only to confirm the opinion that the positions of strata have been changed since their formation, but to give us some insight into the nature and direction of the moving forces.

The simplest case of this nature consists in a mere fracture of the strata, in which the separated parts remain in the same places. This occurs on every possible scale of magnitude; sometimes affecting very small beds, or even portions of rocks; at others, involving a large series of strata. The extent of such fractures also varies; but is rarely very great, without being attended by those motions which will immediately be considered.

It is much more common to find that the separated portions of strata, thus fractured, no longer preserve the same continuity of position; and these changes are attended with some variety. It is observed, in the first place, that a distance intervenes between the separated ends of the mass, in a line, vertical, or nearly so, to the plane of the strata; and the quantity of this, or the extent of the slide, is often considerable, producing those *shifts* which are of such frequent inconvenience in the progress of a coal mine. In such an event, the broken strata may continue parallel, though no longer in the same plane: but it also happens that an alteration of the angle occurs at the same time; so that the same series of rocks presents two different inclinations on the opposite sides of the place of fracture. It needs scarcely here be shown, that, in extreme cases of this nature, the phenomena of opposing or convergent strata may be produced; or that fractures attended with a mere change of inclination, may occur without being accompanied by any sliding or complete separation of the broken ends. To say that such fractures and shifts may also be attended with flexures of the strata, or that they may be the consequences of curvatures exceeding the power of flexibility, would be only to anticipate what is necessarily detailed in the following chapter.

It is usual, in the language of miners, to consider a stratum as either elevated or depressed; but this expression is regulated by his casual progress along the stratum, and by the direction in which he is consequently compelled to seek that part which has disappeared from the line of his progress. But there is a real distinction of this kind; since if, for example, we assume a series which is elevated at a high angle, and trace it from above downwards along its plane, the separated portion, in such a case of fracture, may either be elevated above or depressed below, that part which it has quitted. Appearances of this nature serve to give some insight into the place, at least, of the agents or forces by which the fractures have been produced. In the case of vertical strata which have been broken and shifted, it is evident that the motion must be horizontal; while, in horizontal strata, the change of place can be considered as an elevation or a depression, only as it relates to the progress or position of the miner.

As it has been shown that the changes of position displayed by the stratified rocks have occurred at different periods, so it is certain that fractures and shifts belong also to different æras. This is proved without difficulty, by finding that one of these intersects the other where two occur together; while even more than two are not unfrequent. But we must remain content with knowing this bare fact; as no criterion has been discovered by which it can be ascertained at what period any slip took place, or what distance of time has intervened between successive events of this nature.

The circumstance of fracture proves that the rocks which have thus been shifted, had acquired their induration and rigidity before the slides took place; and as the fact of curvature shows that the rocks affected in this manner were then flexible, it has been argued that curvatures are necessarily of a more remote origin than slides. This opinion must however be qualified; as it is shown in the succeeding chapter, not only that curvatures and fractures may be simultaneous, but that strata are actually bent by the same forces by which they have been broken and separated from their connexions.

All the appearances which belong to fractures and shifts, are to be seen on a small scale, in perfection; occurring in innumerable rocks, so as to form cabinet specimens which are perfect natural models of the great events that have produced so many important changes in the surface of the globe. The varieties thus occurring, are often singularly interesting, from their peculiarities; and, among those that have been described, there is none more remarkable than one of which I have given an account in the *Geological Transactions*. In this, a succession of equal slides has taken place, in such a manner as to separate an intersecting vein of the original rock into numerous parts; marshalling these in a new line which forms a consisiderable angle with the original position of the vein. While the fact is in itself remarkable, it serves as an indication of the changes which must take place in the form of the surface, when slides of an analogous nature have occurred on the large scale.

The phenomena of rock veins having already been described, and those of mineral veins forming the object of a separate chapter, it can only be necessary to remark in this place, that all the displacements which occur in strata, and all the reasonings which apply to their disturbances and motions, equally occur in veins, and are equally applicable to them, of whatever description they may be. As such veins often exist in the rocks, before displacement, it is evident that they must be implicated in all the consequences of the motions of these. The very existence of some veins is further involved in that of fractures, and it is therefore an unavoidable object of consideration in this place.

The fractured ends of a stratum, or rock, may remain in contact, without any intervening material, or they may be separated to a distance in the same manner. But this is a rare occurrence, and is only observed in the more recent fractures and displacements; generally, indeed, in those in which we can almost name the period of fracture. It is a general rule, on the contrary, that some substance intervenes where the separation occurs; and this, according to its quality and thickness, forms veins of different characters and dimensions. It is equally obvious, that intersecting veins will be the natural consequence of fractures and slides occurring at different periods of time.

In the minuter appearances, the matter of the veins is generally quartz or carbonat of lime, sometimes indurated clay: in the larger, conglomerated substances, or portions of the unstratified rocks occur; but enough has already been said on a subject which nevertheless required to be noticed among those phenomena with which it is so intimately connected. I need only here remark, that although the reunion of fractured rocks is generally produced by the intervention of those substances which form the minuter veins, and generally, it is probable, from watery infiltrations of silica or carbonat of lime, the process, in some cases, appears rather more mysterious. Thus I have observed, that hornblende schist is reunited by veins of the same substance, and gneiss by intermediate laminæ of granite. A little examination is however sufficient to show the real nature of these; the new rock which forms the cementing substance, consisting of fragments of the original ones united by infiltrations of quartz.

In terminating this description of the appearances which attend the fractures and shifts of strata, it is proper to inquire respecting the truth of a remark which has been made by philosophers of high reputation, namely, that they are more frequent among the primary than the secondary rocks. I believe it will appear, on an unbiassed investigation, that this is not the fact; neither is its existence necessary for the support of the arguments in aid of which it appears to have been brought forward. Contortions and flexures, it may be admitted, are most frequent and extensive among the most antient rocks; but assuredly none of these can be said to exhibit the derangements under consideration, in a degree superior to some of the latest deposits, to the coal strata, for example. But this distinction is in itself curious, and even perhaps more valuable in argument than the imaginary statement here contradicted; as it proves, that although both kinds bear marks of displacement and force, they have yielded to it according to important differences, either in their original constitution, or in the accompanying circumstances.

The proofs of elevation of the strata given at the commencement of this chapter, are sufficient to account for the occurrence of fractures and shifts; and whatever causes may be assigned for the one, they are, with certain modifications, applicable to the other. It remains, however, to consider what further arguments these phenomena afford towards the existence of motion among the strata.

The bare circumstance of a separation existing between the ends of two sets of strata, in the order and nature of which the most accurate correspondence can still be traced, are proofs of motion that cannot be mistaken. It would be intruding upon the reader's patience to dwell on the further important proofs that are seen, in the great change of inclination between the separated parts, which sometimes occurs in these cases. But it is essential to notice the effects often produced at the places of fracture; which consist in the comminution and separation of those fragments that have afterwards constituted the veins, or which, at least, enter into their composition. Thus, the conglomerate veins noticed in a former chapter, are evidences of the force which has attended the separation of the strata, from the fragments of which they are chiefly formed.

Of the Causes of the Displacements of Strata.

Having already adduced all the arguments necessary for proving that the elevated strata have undergone changes of position, it is proper to examine those cases where, not only the fact of displacement is seen, but where its immediate causes, or perhaps more strictly, concomitants, are present. These are to be found in the appearances which take place at the junction of Granite and the Trap rocks with the strata, the minuter circumstances of which are particularly described in their more proper places, (chapters xxiv. and xxxix.) In these, we can trace examples, not only of every modification of elevation, fracture, and slide which has been described, but the phenomena of curvature also that are considered in the succeeding chapter. The more immediate causes of the dislocations are here obvious; as they consist in the intrusion of masses of matter, which every accompanying circumstance proves to have been introduced in a fluid state. Thus we, at any rate, establish one evident immediate cause for the displacement of rocks, and may with equal security conclude, that the remote one lies in the power of heat. Could there be any doubt respecting the existence of this power, and of its efficacy in producing the effect of elevating the strata, the state of the Coral islands of the Pacific Ocean, which is fully detailed in a subsequent place, (chapter xvii.) would put it beyond dispute. In these, the elevation of masses of coral by the agency of volcanoes, is proved in a manner that cannot for a moment be questioned.

It remains next to inquire into the causes of the elevations or dislocations of strata, in those cases where no immediate evidence of force is present; and to see, lastly, whether any train of analogy can be established throughout this chain of facts, capable of leading us a little further, namely, to the general causes of dislocation and elevation; a question forming one of the most important of those which are included in a theory of the earth.

Although, in many instances, neither the presence of trap nor granite can be traced in the vicinity of elevated and displaced strata, it does not thence follow that they are not present. It is well known that trap rocks have entirely disappeared from the surface in many places; leaving, as indications of their former presence, scattered masses, deep collections of basaltic soil, or veins. Of these facts, Scotland affords innumerable examples which cannot be misconceived. Here, it is plain, we might attribute the displacements of strata which occur in such situations, to unknown or hypothetical causes, when they are in fact the result of well-known actions, and of the former presence of substances whose power we have in other cases witnessed. It is equally certain that the veins of trap are connected with deep-seated masses of the same substances; and it is easy to conceive that these may, not only be themselves invisible, but that even their ordinary indications, through ramifying veins attaining the surface, may not be present, or are, from cicumstances, not to be discovered. Thus strata may have been elevated by subjacent yet invisible masses of the trap rocks.

The same reasoning applies in a still stronger degree to granite, which, from the very nature of its position with respect to all the strata, is often, not only present, but even near at hand, when it is invisible. Of this, we see examples every day in granitic countries; where natural sections sometimes detect the presence below the surface, of masses of granite which do not reach it; or when the presence of continuous massess of this rock beneath, is indicated by veins or by detached portions.

Now in examining the junctions of the strata with granite, it is found, as elsewhere more fully detailed, that these are of a very irregular nature. Not only are they such as to prove that the strata have not been deposited on the granite in regular succession, but the marks of fractures and dislocation which they present, prove that the granitic rocks have been protruded beneath them and have formed the immediately acting force. Thus it may be argued, that in many instances where granite cannot be traced, or where we have not even any indications that should justify us in believing it present, it may still be the cause of the appearances under review.

Thus far the chain of argument appears unbroken, but here it terminates. It is unnecessary to pursue it with the same minuteness through its more hypothetical progress. The strata present marks of elevation and dislocation; it is shown that these appearances are visibly produced by the intrusion of unstratified rocks; it is inferred that they may be produced by the same causes in places where the agents are invisible. It is easy for the reader to complete this process; or, if he is dissatisfied with the consequences that would arise from thus pursuing it, to withhold his further assent. It is incumbent on an author to place the premises on which a theory may be established, in as clear a view and on as firm a foundation as his capacity and knowledge permit; but it is not necessary that he should insist on that assent which he may himself have given, as long as one step of the process remains imperfect or one solid objection can be urged.

In terminating this chapter, it is yet necessary to say, that some geologists have suggested the subsidence of the strata into subterraneous caverns as causes of their elevation and dislocation. After all that has been urged against the power of such accidents to produce the effects of this nature that are visible, I know not that there is more than one variety which it is really incapable of explaining. The force of the gravity of such masses may be conceived equal to any forces of protrusion and expansion that can be assigned; while, excepting that just alluded to, there is no modification of the supposed direction of the protruding force, nor any combination of these, which may not be as adequately represented by the reverse powers of gravitation. This is the case in which masses of strata, often involving large tracts of country, are moved horizontally; a circumstance of frequent occurrence, and clearly proved by the intrusion of vertical veins. In Cornwall, where so many veins are either vertical, or elevated at high angles, this effect must have taken place in a very extensive manner: but the most palpable instance on record is that described as being found in Sky, in the account of the Western Isles. At this place, the vertical veins of trap are so numerous, as, in some places, to equal in breadth one fourth of the including strata: throughout the whole tract, they may be conjectured, without any error that can affect this conclusion, to amount to one twentieth. It is plain therefore, that the mass of strata must have been laterally extended by so much; and it is evident that this cannot be explained by any modification of subsidence, as the sides of the veins are parallel.

Till indeed some evidence is produced of the existence, or at least the probability, of such a cavernous structure in the earth, we might, as we have been desired to do, fairly withhold our assent from an hypothesis which gives us no additional advantages; which does not explain the appearances in an easier manner, and which possesses the further defect of being unconnected with any of the analogies or phenomena by which the theory of subterraneous elevation is supported. Yet I trust hereafter to show (Chap. xxi.) that there are appearances among the strata that cannot be explained in any other manner; and if the proofs then to be offered are admitted, it will be established, not only that there actually exists, or has existed, a cavernous structure of the earth, adapted to the production of displacements by subsidence, but that most important changes of that nature have been produced in this very manner.

CHAP. IX.

On the Flexures and Contortions of Rocks.

As THE fractures and dislocations of strata give rise to important deductions with regard to the changes which have taken place in the superficial portions of the earth since their deposition, so, by their flexures and contortions, we are led to form interesting conclusions respecting the original condition of these deposits, or the changes which they may have experienced after their formation.

Instances of the contortion and flexure of strata have been observed in every part of the world, and they will be found recorded in the writings of numerous authors; of those who have had a theory to support, and of those who profess to be alike indifferent to all. I have without scruple associated together both these modifications of curvature; not however without being aware that some writers have made distinctions, and have deduced separate arguments for their particular purposes, from each. It appears to me that some of these have been warped by the particular bias of the writers; but such distinctions as seem to be essential, or to involve interesting circumstances, will be pointed out wherever it is thought necessary.

Of Flexures.

The simplest species of flexure is that which resembles the marks left by the retiring sea on sandy shores. It is so common that it would be superfluous to quote examples; but it is interesting to point out the kinds of rock in which it has been observed. It is so frequent in the secondary sandstones, that it must have fallen under the notice of every one; and it is often also found in the shales and limestones which belong to the same series of rocks. The cause appears to admit of no doubt; as we are already sure, from the presence of marine shells in the secondary strata, that they have been deposited at the bottom of the sea. There is no difficulty in understanding how those marks should remain in the strata after consolidation; as it will be found that their planes are separated, exactly as those of the level strata are, by changes in the quality of the materials, or by the interposition of another substance; just as the loose quartz sand on the shores suffers mica or fine clay to be deposited on its surface.

The very minute undulations which are sometimes found in the argillaceous schists, differ from these exceedingly, as well in their size as in their regularity; and if they have not arisen from the same cause, it is to be feared that we are not very competent to explain them. To say that they are modifications of the concretionary structure, may perhaps be to approach the truth; since we know of many appearances far more extraordinary than this, which will admit of no other explanation; imperfect as that is, from our total want of acquaintance with the true nature of this mysterious process.

The species of curvature next to be considered, is of a very different nature, and opens a much wider and more important field of inquiry. This sometimes occurs on so large a scale as to involve whole districts, being productive of correspondent undulations in the surface of the land. It is scarcely necessary to say that it varies, as well in the degree as in the disposition; the curvatures differing exceedingly in their length of radius, and being either limited to one, or repeated in successive undulations, so as to present points of contrary flexure. Curvatures of this extensive nature are also found to involve a whole series of strata; extending to depths, of which the limits have not been ascertained. The Coal deposits of Ireland present conspicuous examples of this nature; but the most splendid and satisfactory instance in Britain, is found in the natural and deep section that separates the islands of Jura and Scarba. The whole series of repeated strata, consisting of quartz rock, micaceous schist, and argillaceous schist, is here laid bare in cliffs of many hundred feet in height, and may be examined with ease by those who may choose to encounter the dangers of the famed Coryvrechan.

It does not often happen on this very large scale, that the curvatures of the strata are so considerable as to become coincident with those of lengthened ellipses, or that the opposite sides about the points of flexure approach towards a parallel position. Examples of this nature have however been observed; and the consequences are sometimes such as to give rise to important errors respecting the alternations and relative positions of certain strata. As the superficial rocks of a tract of country are often removed by the consequences of that process which has been called denudation, it is easy to understand how, in such a case of successive acute undulations, all the points of flexure at the surface may have disappeared; leaving the appearance of a succession of strata more or less irregularly inclined to each other, or even, it is possible, absolutely parallel. It is evident that, in such a case, a very simple series would appear to be very complicated, and this, in proportion to the number of such undulations. It is further easy to see, that if this undulation were vertical, or disposed towards the horizon in such a manner that the parallel portions should show a horizontal tendency, a given bed, which was truly the uppermost of the series, might appear at one curvature to be the lowest, or even to alternate with the others in a certain order. It is easy to perceive how this fact affects the question, already considered, which respects the positions and alternations of strata.

It is proper however to remark that such a deception can only arise in the case of one series, where the different members possess, under all circumstances, a conformable, or parallel position. It is impossible therefore, as some careless geologists have supposed, that a secondary stratum can ever appear, from this cause, to lie beneath a primary one; because the undulations of these two classes are not coincident, or conformable. So far is that indeed from being possible, that all the considerable undulations not only belong exclusively to the primary strata, but these, whatever they may be, were chiefly determined long before the secondary had been deposited upon them.

The extreme curvatures and undulations which have thus been considered, may, it is evident, generate fractures, so that the points of contrary direction may represent only the mutual meeting of two corresponding series; a case which has been considered in the preceding chapter. Such fractures must be considered as the necessary consequence of a force urged beyond the power of flexibility in the strata: and that this is the fact, is proved by instances where a stratum, or a series of strata, which is only bent at one point, is broken in another. As this fact is of importance in reasoning respecting the causes of curvature, it is proper to point out one remarkable instance where the fracture and flexure are found co-existing at the same point of curvature; the different effects arising from the different nature of the associated rocks. This example, already quoted for another purpose, may be seen in Lunga, where argillaceous schist and quartz rock are found alternating. Where the flexure is very acute, the quartz rock is broken while the schist is only bent; and, in some extreme cases, fragments of the former are separated and entangled among the latter; facts speaking a language that cannot be misapprehended.

It seems to be admitted that curvatures on the large scale may take place in more than one direction, so that the strata shall represent spheroidal crusts, either concave or convex. I am neither able however to refer to any well-marked instances of this nature, nor aware that they involve any peculiarities which may not easily be conceived.

Curvatures of small dimensions are generally more intricate and violent than the larger, and they abound every where. It will also be found that they are far more frequent in the primary and most antient, than in the more recent strata; and, of this fact, gneiss offers some of the most remarkable examples. It must also be observed, that the parallelism of the approximate strata is not so well preserved in these cases; the curvatures in them differing, either from the originally unequal thickness of the different beds, or because, during the period of softness and motion by which these phenomena are here explained, some parts have undergone a greater extension than the others. Fractures are also a frequent occurrence in these cases; and the union of these two appearances proves, that while the bent stratum was flexible, it still possessed a rigidity limiting its power of yielding, within a certain range.

Of Contortions

Intricacy of flexure naturally leads to contortion; a term properly applicable to the appearances observed in many rocks, and very conspicuously in gneiss and in micaceous schist. Like the more violent degrees of flexure, these contortions have never been found beyond the range of the more antient strata. To convey any idea of their forms and varieties by description, would be impossible, as the imagination can scarcely exceed them. It is not correct to say, as has been said, that these contortions, any more than ordinary flexures, are limited to one of the opposing lines that may be drawn on the plane of a stratum; or that they affect the vertical and not the horizontal direction, as has been equally asserted. A stratum may undulate according to its dip or its direction; it may be contorted in either line, or in both at once. Nor is the imaginary limitation required; as it is not necessary to limit the nature or direction of the force applied.

It is here essential to state, that while the greater flexures are very extensive, whether as they regard the space, or the number of strata affected, the smaller are limited, and the minuter contortions confined to still more narrow bounds. It is possible, for example, in the case of the smaller flexures, that one portion of a stratum may be bent while the other is straight; or even that a bent or undulated stratum may alternate with a straight one. In the case of contortions, the intricacy of curvature is often limited, even to a space of a few feet or inches; or else contortions of so many different kinds and degrees may exist in one rock of no great dimensions, that it is difficult, or impossible, to believe that any force which can be specified should have produced such effects. But these difficulties will be considered hereafter.

There is one peculiar circumstance sometimes attending the minuter contortions, which requires especial notice; because it has given rise to the unfounded notion that the rocks which are commonly admitted to be stratified, also existed in the form of veins. It can only happen where two rocks of different characters approximate; and I shall here select two examples from different cases, because they will serve to explain all similar appearances. Full details v of two particular instances of these have been given in the account of the Western Islands and in the ISL Geological Transactions.

At the points of flexure in an alternation of quartz rock and argillaceous schist, or of any two rocks differing in their degrees of flexibility, the lamina of the soft rock is sometimes so protracted and attenuated as to assume the form of a vein for a limited distance: but to mistake such an appearance for a real vein, requires a degree of incapacity, inattention, or prejudice, which can fall to the lot of few. Similar effects, but from a different cause, are observed where trap rocks invade and disturb the regularity of schistose or arenaceous strata. Minute portions of laminæ become compressed by the invading rock: in the case occurring at Kinnoul hill, they are not only extenuated till they vanish, but they even ramify. Yet no error need arise, even in such a case as this; since the observer may trace these deceptive prolongations to the stratum to which they belong.

There are instances in which the contortions of rocks are not at first obvious to inspection, although they admit of being discovered. This sometimes happens in the case of the schists, which show signs of it only after they have been exposed to the action of heat; if these are not rather examples of the concretionary structure. In other cases, where contortion and a concretionary structure are co-existent, the former may be concealed by the latter; or, while the indications of concretionary structure are marked in the divisibility of the rock, those of the contortion are pointed out only by variety of colour. A mass of soft schist in the Dock-yard at Plymouth, offers an accessible example of this fact; the contortions being indicated by an alternation of different colours at angles to the schistose structure.

Although the most important and remarkable cases of contortion are found in the strata, the unstratified rocks are not exempt from them, and they require to be noticed among the others. From the very nature of these rocks, it is evident that they cannot be easily seen; nor indeed are they to be discovered except from the consequences of decomposition. They are limited to pitchstone and to some members of the trap family. In the former, they are discovered by differences of colour on the surface, resulting from the action of the air; and, in the latter, they are commonly rendered evident only by a further advance towards decomposition. They will require no other observations here; as they are noticed in treating of these rocks, and as their causes are entirely distinct from those which have produced the corresponding effects that are found in the strata.

The last species of contortion calling for remark, is that found in rock veins, and it presents difficulties peculiar to itself; at present, apparently unsurmountable. Micaceous and argillaceous schists are, as is well known, very commonly traversed by veins, often numerous, and generally slender. In the former rock, they invariably, I believe, consist of quartz; and, in the latter, sometimes of this mineral, and, at others, of calcareous spar. When the rocks that contain them are contorted, the veins are at the same time bent; following their sinuosities, however intricate, without any fracture or breach of continuity. However inflexible these minerals may appear, it is plain that they must, in these cases, have undergone flexion. It is impossible, either that open fissures occupying these intricate directions could have been formed in the rock after it was bent, or that, if previously existing and empty, they could have been so bent without being obliterated; waiting to be filled, at a future time, by means of those infiltrations of carbonat of lime or silica which produce them. A case even more difficult sometimes occurs in gneiss; where veins of quartz are found most intricately contorted, although the laminæ of the including rock show no corresponding indications of flexure. These are difficulties which, among a thousand others, must remain for future explanation; and how far the solution of these cases may modify any of our theories, it is impossible at present to foresee.

Of the Rocks subject to Flexures and Contortions.

It was already observed, that contortions were chiefly abundant and conspicuous among the more antient strata; and, of these, they are exceedingly common and remarkable in gneiss, as well as in the hornblende schist by which it is often acompanied. By the alternation of these rocks, which are generally so strongly marked by their differences of colour, they are indeed often rendered visible at a distance where they would otherwise escape notice. They abound also in micaceous schist, where they offer examples of intricacy far more minute than even in gneiss. The flexures of quartz rock are, on the contrary, either on so large a scale as to be insensible, except in a considerable space, or, when they occur on a scale somewhat smaller, the appearances are exceedingly partial. Through the whole of a most extensive examination of the quartz rock of Scotland, I have found but two examples of this kind, and even those were barely sensible in an extent of a few inches. The value of this fact will hereafter be apparent; although it appears in some measure at variance with those already stated respecting the tortuous nature of quartz veins.

It may perhaps be considered remarkable, that although argillaceous schist is subject to flexures, it rarely, if ever, exhibits those intricate contortions found in micaceous schist or gneiss. Yet, in primary limestone, ^ the curvatures are often as great as they are in the last of these rocks; though never exhibiting that extremely tortuous character so often found in micaceous schist.

are rare
written by
I S 2, –

With respect to the whole of the later, or secondary strata, consisting of limestone, shale, and sandstone, the same general remark will suffice. They are all subject to curvatures; but in no case have these been observed to be sudden or considerable, much less, intricate.

Before proceeding to inquire respecting the cause of these appearances, it remains to see whether any of the facts that relate to their situation or connexions, are likely to bear on this question.

It may be remarked in the first place, that the chief curvatures take place in the most antient strata; and, in these, it is observed that their positions are not those which they must have originally possessed, if, as is supposed, they have been deposited at first from water. Yet we must be cautious not to make a rule from this observation; as has been done for the support of a theory, by writers who ought to have been well aware of the injury done to a cause by an inapplicable argument. The secondary strata are sometimes elevated to the vertical angle without curvature.

It is next well known, that curvatures of the strata, and even considerable contortions, occur in the vicinity of trap and granite; while the phenomena by which they are attended, prove that the derangement of the one corresponds to the position of the other. Whatever value may attach to this well known-fact, in the following arguments, it must at the same time be added, that innumerable instances of curvature occur, where the presence of these intruding rocks cannot be discovered.

Of the Causes of Flexures and Contortions.

It remains now to inquire into the causes of these phenomena, which, it must be very apparent, are of high importance in every investigation that has for its object a legitimate theory of the earth.

In the first place, it is proper to ascertain whether the strata may not originally have possessed this form, whatever theory may be received respecting their origin. It may be admitted without much hesitation, that many cases of moderate curvature may have arisen from the form of the basis on which these strata were deposited; as it is not absolutely necessary that their materials should have subsided on a horizontal plane. But it must be apparent that this offers but a very limited resource towards the explanation of the phenomena of curvature.

They who have chosen to imagine that strata have been formed by crystallization, and not by deposition, have also asserted that all the appearances in question were results of that process; supporting the weakness of their arguments, if not founding their opinions, on a casual expression of Saussure, which his better sense and more extensive knowledge would probably not have maintained for an instant, had the question become a matter of real discussion. The laws of crystallization are far too definite to admit of so lax an application of the term. By these laws, the simple atoms, or compound molecules of bodies, are arranged under certain geometrical forms, from which they vary only according to geometrical rules. Chemistry knows nothing of a crystallization proceeding in defiance of these; arranging mixed, not compounded bodies, and disposing of strata, supercompounded in every possible manner, over miles of space, in every capricious mode of disarrangement. Philosophy here recoils to destroy the ignorant hand that attempts to use it. It may perhaps be said that we know nothing of crystallization on the great scale; and that, in phenomena of this extensive nature, it may have operated by other laws than it does in a rhomb of calcareous spar. This is to disclaim philosophy altogether, to make and unmake laws at our pleasure. But, fortunately, it may be proved that this assumption is inconsistent with fact; and that, even on the great scale, the laws of crystallization are consistent. In the island of Coll, there is a vein of graphic granite of great dimensions; and, throughout the whole of it, the felspar, however interrupted by the intermixture of quartz, preserves a common polarity; proving that the entire mass has been subject to the simple geometric rule which determines the form of the fundamental rhomb of felspar. But enough of this gratuitous hypothesis.

In examining the probable cause of flexures, there are two points to be considered, namely, the capacity of the rock to admit of bending, and the power by which this effect was produced.

The present rigidity of the strata obliges us to seek for facts to prove that they have not always been in that state, and I shall here enumerate those which bear on that question. It has elsewhere been shown (Chap. xii.) that water is contained, in perhaps all rocks, and, in many, in considerable quantity; being so loosely united, at the same time, as to be readily dissipated on exposure to the air. Thus minerals, rigid and hard as glass in our cabinets, are often flexible and soft in their native beds; a case which, in my own experience, occurs in asbestos, sahlite, tremolite, and chalcedony, and which is said also to happen in the beryl. It is not unlikely that it will be found far more common, when mineralogists shall themselves collect their specimens from nature, and not from the repositories of dealers. It is not unreasonable to conclude that a state which belongs to the simple minerals, may also belong to their compounds. It is even more probable, when we consider that the union of heterogeneous parts would more readily admit of the water necessary to their flexibility. If it has not been oftener observed in rocks, it is because we have rarely any access to them except near the surface where they have already lost their water, or because we can not procure and examine them within the time necessary to prevent that change from taking place. But it is, in fact, known that many are not only soft, but partially flexible, when wet or first procured from the quarry. This softness indeed occurs in so many rocks, that it is almost unnecessary to point out examples of it. The deep-seated granite veins in the quarries of Rubislaw near Aberdeen, are not only flexible, but so soft as to receive an impression, becoming hard after exposure to air. The well-known limestone of Sunderland is flexible. In Sky, I have seen a sandstone which could be moulded like dough when first found: and there is a sandstone from China, known to mineralogists, which may be compressed by the hand when immersed in water. There is no difficulty in understanding how beds of clay may have been bent without losing their laminar form; and if shales are the produce of these, as cannot be doubted, they may have been indurated as well after curvature as before it. The extent and value of this argument needs not be pursued; nor needs it be inquired how far the addition of heat, if heat has in these cases been present, might facilitate the flexion of strata not yet indurated.

v-214.
-[illegible]
I. 381

Thus it is probable that strata formed under water may have once been flexible: it is probable that many of those which are out of our reach are still capable of admitting curvatures: and it is probable that the presence of water alone is sufficient to fulfil the requisite conditions.

That strata may be bent by fire, we have hourly evidence in the phenomena that take place in furnaces and limekilns, where schistose rocks have been exposed to heat. In the vitrified forts of Scotland, the micaceous schists which have been subjected, apparently, to the long-continued action of a moderate heat, and to the various pressure of the surrounding materials, are often bent, with little loss of their natural characters, so as scarcely to be distinguished from specimens naturally curved. Let the access of oxygen be denied, that the iron may not be oxydated, let sufficient pressure be applied to prevent the disengagement of the gaseous matters which sometimes inflate the stone, and it is probable that no difference would be found between the natural and artificial specimens. Thus then we have a practical proof that fire is adequate to the production of flexibility in rocks, and are supplied with two distinct agents capable of answering the desired conditions.

It is next necessary to inquire what facts can be produced to prove that the strata, the flexibility of which has thus been shown to exist, or to have existed, have been bent by force.

It was shown that where quartz rock and argillaceous schist alternated, the former was broken at the points where the latter was only bent. The fracture here proves the application of force, of that power which bent the flexible stratum. In innumerable cases, the same strata, flexible enough to yield in a limited degree, but rigid beyond a certain point, have submitted to the moderate curvature, but have broken where they could bend no longer. Facts like these can leave no doubt respecting the exertion of a force producing both these effects.

When strata are disturbed by the intrusion of trap or granite, or by the passage of veins of those rocks, they sometimes present traces of fracture, at others of flexure, and, not unfrequently, of both united. Here, the operation of force is equally evident as in the preceding cases, while the very nature of that force is also apparent. The cause of the softening of the strata is here also probably to be sought in that heat to which the very origin of these intruding rocks must be attributed, and of which the effects are also the moving powers in question. The case from Lunga, already quoted, is of this nature. The north-east coast of Sky presents also examples of the same kind, which are particularly explicit and interesting; as well from their number as from their variety and distinctness. In the clear and extensive natural sections produced by these lofty cliffs, the original and undisturbed condition of the level strata, consisting of limestone, shale, and sandstone, is in many places traced with the same ease as if they were exhibited in a model. The intrusion of the trap is equally visible in others; presenting every modification of the joint disposition of these different classes of rock, that has yet been recorded wherever these phenomena have been observed. Here, the marks of force exerted are seen under numerous shapes; and, among others, huge fragments and portions of strata are found, not merely broken, but far removed from their original connexions, and, in many places, incurvated to a considerable degree. These occurrences, not only therefore prove the actual existence of moving forces, and show the extent and nature of their power, but, at the same time, demonstrate that the heat which accompanied the intrusion of the trap, rendered the strata flexible enough to yield to the power which it exerted.

That force has been exerted in the flexure of strata, is further rendered probable by the circumstance, already mentioned, respecting the greater frequency and more violent nature of the curvatures that occur in the more antient strata, which, from causes sufficiently apparent, have undergone those greater changes of position that mark the extent and magnitude of the motions which have been impressed on them. Facts, elsewhere noticed, respecting the composition, nature, and origin of these rocks, seem also to justify the notion, that in these cases also, the flexibility and the curvature have been, both, equally the consequences of the action of heat. The argument, in this case, rests precisely on the same basis as it does in those where curvatures of the adjoining strata attend the intrusion of masses or veins of trap; and, in both cases, the force exerted, and the capacity to suffer from it, are equally the effects of a common cause. It is here of course concluded, as is fully argued and, I trust, proved elsewhere, that whatever is received respecting the nature and origin of the trap rocks, cannot be denied to those of granite.

The gneiss which forms Cape Rath, elsewhere noticed, is traversed by innumerable granite veins; by the interference of which the beds of that rock and of the accompanying hornblende schist, are broken into fragments and entangled among a mass of veins of different dates, so considerable as far to exceed in quantity the rocks which these have invaded. In many parts of this compound mass, the detached portions of the hornblende schist, rendered peculiarly visible by their colour, are bent in various ways; presenting striking examples of their capacity to admit flexure, and indicating in a way that cannot be disputed, any more than in the analogous cases of trap, the nature of the cause which effected, both the curvature and the displacement.

The last argument which I need offer to prove that the flexion of strata has been the result of force impressed on a flexible body, and not a consequence of any original disposition or concretionary structure, is found in those rocks which, like micaceous schist, present a certain parallelism in the position of one or more of their integrant minerals.

When mica occurs in flat beds of gneiss and micaceous schist, it is found invariably to be more or less accurately parallel to the planes of the strata, and, consequently, to itself. There are two ways in which this appearance may be explained; both of which form objects of inquiry in another part of this work. The mica may have either assumed this disposition from mechanical causes, as happens in the later sandstones, and even in the loose sand of sea shores, or that position may be the result of a common polarity, as is evinced in the parallelism of the hypersthene in some varieties of hypersthene rock, or in that of the mica in certain trap veins of rare occurrence. Under either supposition, the present argument is equally valid, so that its consequences cannot be evaded. Now when gneiss or micaceous schist is contorted, it is observed that the mica is no longer parallel to itself or to any level plane that may be assumed in the rock, although it still continues to be parallel to the curvatures of the beds. Under the theory of deposition, it could not have assumed the various and extraordinary situations it often presents; it could

Mica
in Trap
veins
v II/152

not, for example, be found in a vertical position. If again the contorted forms of these rocks are conceived to have arisen, as well as the straight, from crystallization, it is plain that the law of polarity, which is in one case preserved, is in the other violated. In either case, the curvature of the strata from actions posterior to the disposition of their constituent minerals, is proved by evidence that admits neither of evasion nor dispute.

It now remains only to inquire respecting the nature and actions of the forces which have produced these effects.

The evidences of change of place in the strata after deposition, have already been stated; and it has been shown from the phenomena of trap rocks, and from other appearances, that we can even form probable conjectures respecting the nature and cause of some of these moving powers. On these subjects there is nothing further to be added in this place.

It is not easy, however, always to specify the manner in which these forces have acted in producing the curvatures of strata. In some cases, it may be imagined that the mere force of gravity, acting on a flexible mass unequally supported, may have been a sufficient cause; or, that under the same circumstances, strata may have been bent by the action of those subterrene expansive forces which have elevated the coral rocks of the Pacific high above the level of that sea in which they were produced. But the cases of highly undulated strata with alternate points of contrary flexure, appear to require a lateral pressure exerted on highly flexible strata, in a manner of which we can form no distinct conception. Still less can we assign the precise action of forces capable of producing complicated contortions, and more especially in those cases where they are limited to one stratum out of many, or to one part of a mass while the other appears undisturbed. Yet amidst the innumerable marks of discordant and repeated changes exhibited on the surface of the earth, all bespeaking the frequent renewals of disturbances, acting also perhaps through periods of time of which we can form no notion, it is not easy to imagine any change in the forms of flexible strata which these may not have produced. This is not the only case in geology in which we must be content to admit of varying effects from a varying cause; without thinking it necessary to reject the general principle, merely because we cannot apply it to every minute particular which is included in them.

CHAP. X.

On the Characters and Disposition of unstratified Rocks and Veins.

The unstratified rocks are far less numerous than the stratified, and they have all been comprised, in the present arrangement, under two general heads. Granite forms one of these; and, to the other, the term Trap has been applied; with some latitude, it must be owned, but to avoid the necessity of inventing a new one. Under these two heads, in the twenty-fourth and thirty-ninth chapters, and in the Classification of Rocks, will be found all the minuter particulars which it was not necessary to introduce into this very general view.

Of the Extent and Places of the unstratified Rocks.

Though the unstratified rocks form but a small part of those which are visible to us, there is reason to imagine, that at depths to which we have not penetrated, they occupy a much greater extent than would be suspected on a superficial view. Conjectures have been offered respecting the proportion of space which granite fills at the surface; but no measurement has been given; nor has any thing been done towards answering this question as it relates to the later unstratified substances. In fact, these vary so much in different countries, that if any worthy object were to be gained by that knowledge, it could still only be done after the whole surface of the globe should have been surveyed and reduced to a geological map. It would be now easy to solve this problem as far as relates to Britain, as well as to some other European districts; but a general expression of the extreme disproportion between the stratified and unstratified rocks, is all that can here be necessary. In Britain it may be stated that the whole of the latter, comprising alike trap and granite, do not cover a thousandth part of the superficies of the island. If we examine the flat tracts of Russia and Poland, and of other similar countries, thousands of square miles will be found not containing the least vestige of these rocks.

Although the unstratified rocks do not necessarily occupy the summits of a country, they are found principally in mountainous, or at least in hilly regions. In the tracts of an uniform low level, to which I have just alluded, they very rarely exist at all. Thus a connexion is traced between the existence of these rocks and the disposition of the surface; a connexion, it is true, not very rigid, but sufficient, when combined with many other circumstances relating to them, to be of some value in our reasonings respecting their origin and consequences.

It is a familiar observation, that granite forms the highest peaks and ridges of the most elevated mountains of the globe. The remark is however more common than true: it is certain that it constitutes many of these, but there are numerous mountains in the first class of elevations, that are formed of stratified rocks to the summits. Even where granite exists, it often forms only a portion of the highest points; the sides, even at very great elevations, and many of the ridges and peaks, being still constructed out of the superincumbent strata. In a certain limited sense, it may also be said, that Trap forms the higher summits in mountainous and hilly regions; a remark very conspicuously true in some parts of the enormous ridges of South America. But granite is also found, even on the level of the sea, in the same districts, as are the different members of the trap family. The theory which will hereafter be given respecting the origin of these rocks, will be found to agree with this mode of disposition.

When the superficial disposition of the strata shall hereafter be examined, it will be seen, that not only a series of them, but a single bed, often occupies a very considerable and continuous extent of surface; and when, from the occasional superposition of other beds, or the absence of portions from the effects of waste, they cannot be absolutely traced, still, their former extent, or even their present, is easily inferred by the well-known rules of geology, namely, the comparison of characters, relative position, and angles of inclination. But the unstratified rocks are rarely found occupying extensive continuous spaces on the surface; being, on the contrary, interrupted by the intervention of portions of strata, or else appearing in distinct, and often in small eminences scattered through a district, of which the fundamental part is stratified. The minuteness of these is often indeed very remarkable; since, in Scotland, where both these classes of rocks abound, it is not unusual to find a portion of granite, of a very few yards in diameter, separated by many miles from any other of the same rock; while the same is frequently observed with respect to trap.

The cause, in these two cases, is, however, different in its nature. By examining sections and tracing the condition of the insulated masses of granite and of the conterminous strata, it is discovered that this rock exists in a continuous state beneath, and that its apparent insulation is the result of the absence of the superincumbent strata at those particular points. In the case of trap, on the contrary, where there are not reasons for supposing that the deposits have been partial, it is often easy to discover that their insulation arises from their own waste; the thinner or feebler connecting portions having been destroyed while the others have remained.

It is easily inferred from the above remarks, that, of the unstratified rocks, one set is inferior to all the strata, and another superior to them. But with respect to trap, at least, this is not exclusively true: whether it be the case with granite, is a question that will be better discussed in another place. The rocks of that division are found beneath the strata, as well as above them, in masses so large and so irregular that they cannot be ranked among veins, otherwise than as every finite mass included among stratified rocks may claim to be considered as such. If a mass of trap, indefinite downwards, should be found beneath the strata, then, by the postulate on which the distinction adopted in this work is founded, it becomes granite. That question being connected with the one which relates to the possible superincumbent position of granite, will be considered in a more proper place. (Chap. xxiv.)

The forms of all the unstratified rocks are irregular, and, in the majority of instances, totally dissimilar from those of strata. It is owing to this absence of the stratified form and disposition, that we are unable to infer the continuity of the rocks under review, when that is not actually visible. It may be supposed, it is true, that if granite exists beneath all the strata, so as to form their immediate foundation, every mass is really continuous; however deep that connexion may be situated. But, with respect to trap, no such inference can be made. Separate portions may originally have been separate deposits; as similarity of character is insufficient to establish their original identity, and as they do not admit of being compared by positions or inclinations; tests which, from their origin and forms, are inapplicable to them.

Of the Antiquity of the unstratified Rocks.

The relative antiquity of the members in any continuous series of strata, is readily known from their order of succession. In these, geology even teaches us to distinguish certain revolutions, marking very different and distinct periods or portions of time during which they were formed. But we have no similar criterion by which to judge of the relative antiquity of unstratified rocks that occur together; though possessing one, sometimes, when a vein from one traverses the mass of another; while, in our judgments respecting the relative antiquity of any of them to the stratified, we must be guided by rules which can only determine that question within certain limits.

With regard to granite, it would be imagined, on a superficial view, as it actually has been maintained, that because it is the lowest it is therefore the most antient rock. This is an error arising, partly from unduly extending the law of succession as it relates to the stratified rocks, and partly from overlooking the obvious phenomena which attend that substance. It is unnecessary to add the moral reasons, arising from habit, authority, hypothesis, or unwillingness to acknowledge error. The detailed arguments on this subject will be found in the history of that rock; but I may here remark, that it is chiefly by the passage of veins from a mass of granite through the incumbent strata, that its relative posteriority to these is indicated. It would be attempting to demonstrate a self-evident proposition, to say, that of two coexisting rocks, of which the one traverses the other, the traversing body is the latest in origin. That such a vein is a prolongation from a mass of granite, renders the posteriority of the latter equally an axiom.

Judging by this criterion, a given mass of granite is posterior in origin to every stratum in a series which is traversed by its veins, or, what is equivalent, is disturbed by its contiguity. Mere contiguity without disturbance will not prove it; as strata might be deposited on granite, and as it is elsewhere shown that they have actually been so deposited. But although this defines the highest limit of relative antiquity, it leaves the lowest indefinite; since, in a deep series of strata, the superior, or distant, portions may have been but slightly disturbed, or have entirely escaped disturbance by a granite which has not emitted its veins far beyond its immediate boundary. However certain therefore it may be, that any mass of granite is posterior to the gneiss, the micaceous schist, or the argillaceous schists which it traverses, or into which it intrudes, we are unable to prove that it is not also posterior even to the secondary strata that lie above them, except in those cases already mentioned where the actual contact is visible. If therefore the elevation of the primary strata from the horizontal to the inclined position is to be attributed to the formation of granite, there is no absolute proof to be drawn, from the nonexistence of that rock among the secondary strata, that these also have not been elevated by the same cause. If, in any case of this nature, we cannot prove that a given effect originates in a proposed cause, it is still important to show that such a connexion is not impossible.

It is evident now, that the relative position of any two masses of granite to particular strata in the general series, can afford no proofs respecting the relative antiquity of these masses to each other. The veins of the most recent may have been limited, like those of the most antient, to the lowest stratum in the series. That criterion must be sought, as I have just said, by inquiring whether the veins of one mass of granite penetrate another. And as this will hereafter be shown to be the case, it becomes proved that granite has been produced at distinct successive periods. It appears that even three periods can be proved; but it must be evident, that, from numberless causes which cannot here be considered, the difficulty of proving successive productions of it must increase at each stage.

The fact of even two generations of granite, is however important in that view which considers it as having been the immediate cause or concomitant of the elevation of the strata. It is shown in its proper place, (chapters viii. and xxi.) that the strata have actually been elevated at successive periods, in such a manner as to prove that the inferior series was at least twice moved; and the theory of granite provides the means of producing both the effects as easily as the one.

The question of the relative antiquity of Trap rests on somewhat different grounds, and requires a separate explanation. In some place or other, the veins of this substance have been found penetrating every rock from granite upwards; as far at least as the boundaries of those strata which have certainly been deposited under the ocean. Hitherto, they terminate with the chalk stratum. At the same time, the masses of that rock lie above this substance, and thus its posteriority to all the strata beneath the tertiary deposits, taking chalk as the uppermost of these, is established. But as trap occupies the surface, and as the upper strata in the general series are not found at every point there, it may of course happen, that instead of chalk, this rock may repose on any other member of the series, as it may even on granite. It must already be apparent, that the mere existence of trap veins in granite or gneiss, cannot prove their antiquity; unless it can be shown at the same time, that they stop suddenly at the point of superposition of some other rock. Neither can the separate superposition of a mass of the same substance on those, or on any other rocks, prove their affinity to these in point of age. That can be inferred only in cases of continuous stratification among the stratified substances; and, if the contrary opinion has been held, it has arisen from a false view of the nature of the trap rocks. Were a continuous mass of trap to be deposited now over the whole of Britain, it would be in contact with every rock in the system. Should certain portions of it disappear through the lapse of ages, there would remain others, independent, and, under occasional circumstances, in contact with granite, with argillaceous schist, or with chalk, as it might happen. Posterity would judge falsely in considering these as distinct deposits, connected, in point of age, with the several rocks on which they immediately repose, and through which each of them would also exclusively send its veins. But this is a picture of the past, as all geology proves; and we are therefore left without a criterion to determine the relative age of any independent mass of trap to the strata, as far as any evidence can be deduced from mere contact. It is important to rectify an error which has led to the establishment of numerical and successive *flœtz trap formations.*

The circumstances, however, under which the trap rocks have actually originated and under which they are displayed, render it certain that there have been distinct deposits at different periods; but it is not less essential, in establishing a fact, to make use of the true evidence, as the false is always in danger of misleading or of proving too much. From the constitution of the porphyries that accompany the older rocks, it is probable that they are of a higher antiquity than the traps found above the secondary strata; but the certainty of two, if not of more successive productions of these rocks, is established by the same fact as it is in granite, namely, by finding veins passing from one mass so as to intersect another. The same is proved by finding masses of trap constituting portions of the conglomerate strata; but all the minute particulars which relate to these appearances belong properly to the history of Trap, where they will be found. (Chap. xxxix.)

On the Veins belonging to the unstratified Rocks.

As the veins of granite and Trap are of a distinct origin from the other rock veins, it will be better to consider them in this place; as far at least as it is necessary for the purposes of this general view of the unstratified rocks.

If veins consisting of granite or trap, have not always been traced to masses of these rocks, that has been done in at least the far greater number of instances, where the attempts have been properly made. That this may not always be possible, is easily conceived: the granite mass may be covered by the strata, and the mass of trap may have disappeared. As trap is also, like granite, a subterranean production, it is easy to suppose that veins of it may, like those of granite, be connected with invisible deep-seated masses. This very fact is proved by phenomena occurring in Sky, which will be more amply described hereafter. It is further necessary to observe, that this apparent independence of these veins has occurred only where they were of great size; that being the sole condition under which they could exist at great distances from the parent mass. I ought perhaps here to add, that I have traced the sources of all those veins in Scotland which were formerly supposed to be independent. It will not be an illegitimate conclusion, that all such veins are processes from masses of those rocks to which they belong in character and constitution.

The sizes of these veins are infinitely various, whether we regard their breadth or longitudinal extent. They have been observed to reach to many yards, and to decend from that to the finest filaments. In their length they also vary; but it must be remarked that the courses of trap veins are far more extensive than those of granite. The larger ones of this nature are known sometimes to extend through spaces of many miles. It has sometimes been imagined that they were unlimited downwards; in which case every vein must be a lamina. That however is not the fact in granite, in which every dimension, even of the largest, can sometimes be easily traced. It is equally untrue of trap; as the lateral boundaries of their planes may be seen and examined throughout, in many parts of the Western Islands of Scotland. Neither can it be true of either, if we consider their connexions. If indeed the depth of vertical trap veins cannot in some instances be traced, it only proves that the masses in which they originate, and which form their limits, are innacessible to us.

The ramification of veins is a circumstance which requires notice in treating of their dimensions. It is extremely common in granite, but rare in trap; from causes that will be so apparent in the progress of this discussion as to require no specific explanation.

The position of veins with respect to the horizon is infinitely varied, as it also is with regard to the intersected strata. Those of granite are generally exceedingly inconstant, while they also pursue tortuous and intricate directions which defy all attempts to follow them. It is only where they are of large dimensions, and hold long courses, that they appear straight; and, in these cases, they seem to maintain high angles towards the horizon. But their direction and extent are, in fact, regulated by the magnitude and disposition of the fissures which they occupy, and by the position of the central mass. If, in Cornwall, the prevailing and larger granite veins hold a common course approaching to the parallel, and if the high angles predominate, it only shows that circumstances in the nature and position of the invaded strata, and in the bulk and form of the invading mass of granite, determined this particular disposition in that place. No general rules can be deduced from any fact of this nature.

With respect to trap veins, it is a remarkable fact that they are rarely congregated into one spot, in minute ramifications; although I have elsewhere shown that this is the case in Barra. On the contrary, the predominant veins of this substance are generally of considerable, and sometimes of enormous dimensions, while they continue to hold extensive courses without ramification; a fact rarely occurring in the veins of granite. This circumstance implies one, or both, of two things in which the generation of granite and trap veins have differed; namely, a different condition of the invaded strata, and a force exerted in a different manner. Perhaps both of these are correlative propositions. Numerous circumstances, elsewhere described, prove a flexible condition once existing in many strata; and most conspicuous in the older ones. These are the rocks which granite has especially invaded; and such limited and tortuous fissures are precisely what might be expected from an imperfect rigidity of the strata. The strata invaded by trap, on the contrary, rarely contain indications of flexibility; and thus are accounted for the greater decision and straightness of the fissures into which the liquid rock has flowed. It is interesting to remark how well the several parts of a system hold together: and how readily a true theory is applicable to phenomena which had not entered into that mass of facts on which it was founded.

Most apposite confirmations of this view of the different effect of fissures on flexible and on rigid strata, and on those which yield most easily compared to those that oppose the greatest resistance, are contained in some of the veins of the Western Islands, as in Lunga and elsewhere. One of them was already necessarily noticed, in the remarks on the contortions of rocks; but while they show the different effects produced on different strata, they serve to illustrate differences in the forms of trap and of granite veins, which are of a very important nature. In one of these, already mentioned, the vein has intersected an alternating series of argillaceous schist and quartz rock; breaking the latter in a decided manner, but bending the former, which consequently, bears the same marks of flexure that are seen where granite traverses gneiss. In the others, there is a striking difference in the forms of the veins where the fissure has been transverse to the laminæ of the invaded schist and where it has conformed to these; the vein having the usual parallel form of trap in the former case, and resembling the irregular veins of granite in the latter.

As the veins of trap are most frequently of large dimensions, so they are, generally, either vertical or inclined at high angles to the horizon. Thus probably they indicate, by both circumstances, the great depths at which the parent masses lie, and the nature and place of the force which produced the fissures. But that they frequently also occupy very low angles, will immediately be shown, when the positions of veins with respect to the strata are considered.

Although the term fracture implies that the strata are disunited in some direction across their laminar arrangement, it does not follow that all veins hold courses thus intersecting these. While they may be broken at any degree of obliquity, they are sometimes also separated according to the planes of their stratification, or according to that of their laminar structure. In the latter case, the veins are rarely large, and are more or less parallel to the laminæ of the invaded rock; in the former, strata, either similar or dissimilar, are sometimes completely separated, even for considerable spaces, so as to permit a large and parallel mass of the intruding rock to occupy the vacuity. The first of these cases is most common with granite, the latter with trap; although, even in this last rock, veins are sometimes found intruding into the schistose structure, or parallel to the stratification of the conterminous strata. In both cases, an incorrect or prejudiced view of these appearances has given rise to the notion of trap and granite being stratified; with what truth will easily be judged, when the following obvious marks by which these are to be distinguished from real strata, are described.

Where the courses of granite veins are parallel to the laminæ of the including rock, their true nature is easily distinguished; as they rarely persist, even for a few inches, without some deviation or ramification. In the case of trap, however, as they are often of considerable extent, it requires more care to trace them to their irregularities or terminations. But they present other indications of their real nature, by occasionally sending out branches, or by the effects which they have produced on the adjoining strata. The nature of the rock indeed, as proved from other appearances, ought to be sufficient to satisfy the spectator that they are veins, and not beds: but as it is probable that real strata of trap do actually occur, I must reserve the minute parts of this discussion to the history of the trap rocks.

Respecting the relation in point of time between these rock veins and mineral, or metallic veins, it is not probable that any general rules can be laid down. In Cornwall, granite veins, as well as veins of porphyry traverse the mineral veins; but as mineral veins are also found in granite, even in that district, it is certain that they are of a time intermediate between two formations of that rock. That ordinary trap veins should generally be posterior to all mineral veins, follows, of course, from the period at which these appear, relatively, to have been formed.

Many chemical and mechanical changes occur in the including strata at the places where they are traversed by veins, whether these be of trap or granite. But these are numerous and minute, and will be required hereafter for the purpose of establishing the igneous origin of those two rocks. But, in this place, where the question of veins has been treated generally, as far as that was possible, it is important to observe that the effects are, in both instances, not only similar, but as exactly identical as could be expected when the various differing circumstances attending them are considered.

On the Origin of the unstratified Rocks.

The reasons for believing that all the unstratified rocks are alike of igneous origin, or that they are substances crystallized from a fluid of fusion, will be given in a more proper place hereafter. The following remarks on the mode in which these rocks assumed their present forms, and on the probable causes and effects of their fluid state, proceed on this supposition. In a science in which there is so universal a reaction of all the parts, and where every set of facts is necessary to illustrate every other, all cannot have that first place in consideration to which so many have a claim.

As it is apparent that granite has been in a state of fluidity beneath the strata, and that, during this state, these have been elevated in an irregular manner, it is easy to account for the irregularity of its general surface, or for the partial way in which it is found distributed on the earth's superficies. The consequence of the unequal elevation of the strata, was to produce those interior inequalities that have been filled by the yielding mass which was the immediate cause of that fracture, and the concomitant of the force exerted. The production of the veins is another obvious consequence of the fractures or discontinuities formed by the displacements of the strata. It must be remembered here, however, that the actual appearance of granite at the surface of the earth is, in most cases, the consequence of another train of effects, consisting in the waste of those parts of the strata by which it was once covered; a waste, of which the whole globe produces the most unquestionable evidence. From the progressive state of that waste, it follows, that the apparent quantity of granite must be constantly increasing, although itself is subject to decay; and if it really be the basis of all the stratified rocks, it is possible to conceive that the earth might, at some future day, contain granite, only, in its more elevated portions; while this could not happen, of course, at low elevations or near the level of the sea, because, here, the ordinary causes of waste cease to act.

Since now the other division of the unstratified rocks is found above the strata rather than below, it is necessary to inquire respecting its source or origin. That it was produced in a fluid state, and consolidated from that condition, rests on precisely the same ground as the case of granite. The nature of the different substances is similar, often identical, the effects are the same on the including strata, and the disposition of the veins is strictly analogous, varying only according to circumstances which have already been stated. Nor is trap exclusively superficial; since it is actually found beneath the strata in considerable masses, or else in such a relative position to them vertically, as to resemble granite in this respect; while, it will further be shown hereafter, that many of the extensive masses of these rocks now visible at the surface, have actually been once covered by strata which have disappeared in the progress of decay.

It would be an obvious, yet a superficial conclusion, that the trap rocks have been deposited, like the accompanying strata, from above. But the moment that their igneous origin is admitted, that opinion falls to the ground; as there is no external source whence they could have been derived. By the argument of dilemma therefore, we must seek their origin in the same regions that produced granite. That this really is their origin, is further proved by the positive arguments derived from the masses that lie beneath or among the strata, by the depth and magnitude of their veins, and by the marks of force which accompany their juxtaposition to the strata. If any further doubt could exist, it would be removed by the phenomena of volcanoes. The substances which these produce are not only strictly analogous in all their essential characters to some of the trap rocks, but often undistinguishable; while the variations which do appear, admit of an easy explanation, from differences easily defined. These rocks, having passed through the strata, flow over them in certain cases; while, in many others, there is reason to suppose, from the effects following the earthquakes which accompany them, that they have intruded among the strata beneath the surface, so as to have produced those well-known permanent elevations of the land found in volcanic countries. That they elevate the superficial strata, is fully proved by the phenomena attending the volcanic Coral islands.

It is in the deeper regions of the globe, therefore, in those where we have found the origin of granite, that we must seek that of trap. These substances are essentially of the same nature, but they have been produced at distant periods of time. In accounting for the present superficial position of trap, we are provided with two resources; that of its flowing out in the manner of lava, so as to cover the strata, and the final removal of these so as to leave bare that which was once concealed beneath them. This question is worth pursuing a little further.

If chalk be the uppermost marine stratum in any spot, and if a trap has been erupted under the sea, as many appearances indicate, it must have flowed over the surface of that rock. It may have flowed above many others; as it is by no means certain, nor even probable, that chalk has been formed every where. But respecting this we cannot hope to acquire any satisfactory evidence; as, in the present state of the earth's surface, it is impossible to determine which strata have been removed, and which were never deposited.

On this supposition we may also account for the partial existence of the trap rocks. It is not necessary to suppose that they have been universal, or even much more general than their present remaining indications now show. Although the materials of trap may have existed beneath, as generally as those of granite have been supposed to do, it does not follow that they should every where have reached the surface. The analogy of volcanoes here comes again to our aid; indicating, by facts which cannot be disputed, the possibility, or rather the probability, of this supposition. It is on this ground also that we may readily admit successive formations of trap rocks; even if we had not those proofs of it which have already been mentioned: and it may thus even, prove, that many of those now known, which are in contact, separately, with different strata in the series, may actually have been of different periods, and not the consequences of a single deposit on rocks of different natures; separated, itself, into distinct parts, by the effects of waste. It is not a little remarkable, on this view, that two deposits of trap, of which the distant succession is proved by appearances that will hereafter be fully detailed, are found in the same place; a phenomenon exactly similar to the renewal of volcanoes at distant intervals among the ruins of those long since extinct.

It must thus be apparent, that whatever differences may exist between trap and granite, whether in their relations to the strata, or their mineral characters, they are strikingly analogous in almost every essential general circumstance, and that the former may, in a certain sense, be considered as a recent granite; as the granite of the newer strata. The circumstances respecting these analogies which have not yet been noticed, will be found in their history in future chapters. It remains to account for one difference, on which much stress has been laid, not only by those who deny the igneous origin of every rock, but by those who, unable longer to blind themselves respecting that of trap, reserve all their force in favour of the aqueous origin of granite.

It has been said that granite, in a state of fusion and protrusion, ought to have been erupted so as to flow over the strata, and that, like the trap rocks, it ought now to be found in that situation; which it is not. This is a proposition apparently very simple; but the simplicity of a proposition ought to be well ascertained before it is advanced as such.

In the phenomena that attend volcanoes, which were just noticed, it seems certain that volcanic matter is sometimes moved, or perhaps introduced among the strata, without appearing at the surface. The cause is here indicated by that eruption of rock which takes place at a distant point at the same time. The condition of the surface in many parts of Italy, and the antient changes of level which it has undergone, seem to bespeak the agency of this cause, if it were not proved, in many other cases, by those alterations in the form of the surface which have attended earthquakes. The elevation of Coral islands proves that additions of solid matter have been made to the strata beneath them, as there is no other mode of solving this phenomenon; while the actual eruption of volcanoes in the neighbouring parts of the ocean, leave no doubt with respect to the nature of the cause.

If, in such cases as this, we conceive the surface to be removed, as it will be at some future period, we should probably find many places where the appearances would resemble those produced by granite; that is to say, masses of fused matter which have not overflowed the strata.

The next step is to apply this reasoning to the trap rocks. If, in any place, the superficial masses of these were to be entirely removed, we should discover the openings whence they had flowed; as we have already probably done, in many cases, by ascertaining the places of veins. Here then we should find, what perhaps we have already truly found, the unerupted trap, with the strata reclining against it, as in the case of granite. If we conceive that, by a still further process of waste, not only the erupted trap, but the subjacent strata themselves should have disappeared, we should arrive at the fundamental mass, and find only the slender remains of highly disturbed strata, covering this interminable source of the erupted rocks. Trap would then be to us as granite; nor would any proofs of its former eruption and overflowing remain.

Now this is not a mere postulate. In the district of Morven, a mountainous mass of trap, attaining to 1200 or 1500 feet in height, meets a similar mountain of gneiss in a line not far deviating from the perpendicular, its base being lost beneath the sea. Here the gneiss reposes on, or meets the trap, precisely as it would a mass of granite, and is in the same manner disturbed at the junction. This trap mass is indeed connected with a portion that covers secondary strata, and which may be considered as its erupted part. But this is fast wasting away; and the time, however distant, must at length arrive, when the trap of Morven will present all the geological appearances of granite, and when, should it accidentally have possessed the granitic mineral character of some of the syenitic traps of Sky, it would be supposed an unerupted rock, and a granite.

Similar appearances occur in Sky, where masses of trap that, as far as we can discover, are interminable downwards, pass through the secondary strata, which are consequently found in the same relative position to them that the primary strata sometimes are to granite. One of these is many miles in diameter; and, did neither Sky nor any other district preserve the vestiges of erupted and overflowing Trap, it might here also be argued that such a mass could not have been ejected from below.

In these last cases, there is one circumstance worthy of notice in this question: it is the difference between the manner in which the trap meets the secondary strata, and that in which granite meets the primary. The junctions in Sky are, in many parts, vertical and very precise; although there are the usual limited disturbances and penetrating veins. This difference arises from a different condition in the invaded strata, which is confirmed by every other circumstance of difference that attends the passage of veins in both, the appearances at the junctions, and those of the strata themselves. These were noticed before in the case of veins, and they arise from the yielding and flexible nature of the strata in the one case, and their rigidity in the other; facts fully evinced by the contortions which attend the primary stratified rocks. The very quality of the rocks themselves, independently of softness, may have contributed to these differences; as is proved by the instance in Morven, where the lateral junction of the trap with the gneiss is of a very different character from those which take place between it and the secondary strata of Sky. So far therefore from its being proved that the mode of junction between granite and the incumbent strata is not the consequence of eruption, because it does not resemble the junctions of trap in similar cases, these differences are precisely of a nature to confirm that opinion. If, in mechanics, unequal bodies are projected with a common velocity, we infer that two forces have acted; if the velocities are different, we then inquire whether one force may not have acted on both. It is not probable that Geology will ever be ranked among the accurate sciences; but we may here infer, from a balance of phenomena, that which we cannot demonstrate.

There is yet a case of unerupted trap, of which Sky produces a distinct example, and which is relevant to the question under consideration. It has also been observed elsewhere. In this instance, a mass of trap, unterminated downwards, and of a conoidal form, is seen, in a fair and deep section, covered by the secondary strata, which are so bent over it as to be accommodated to its form. Here is a case precisely analogous to that of an unerupted granite; and it is obvious, that when, in the progress of waste, its summit shall reach the surface, it will present an example of unerupted trap, with the strata conforming to it on each side, just as the primary are so often found to do in mountains of granite.

We may now inquire how these facts apply to the state in which granite is supposed, by the postulate, to be invariably found; unerupted, and not lying on the strata to which it is contiguous.

The last case of trap quoted, is here applicable, and perhaps more completely than would at first be imagined. If it was not necessary that a protruding and protruded mass of trap should in every instance make its way through the superincumbent strata, neither has it been in the case of granite. Even the rigid sandstones of Sky have yielded to the pressure of the trap; and much more must it be believed that this has happened in the case of granite, when the softness and flexibility of the primary strata in its vicinity are considered. I need scarcely add that the volcanic elevations of strata, without eruption, present the same analogies.

There are moreover circumstances in the constitution of granite, which prove that it has, for a longer period, been under the influence of heat than the trap rocks. This is its more perfect and distinct crystallization; a difference which we are enabled to produce, even in our laboratories, by prolonging the fluid state of fused traps, and which is a common occurrence in volcanic rocks under similar differences in the rapidity of cooling. I shall hereafter also attempt to show, that gneiss itself is the evanescent limit between granite and the other stratified rocks. Thus it is at least possible, perhaps even probable, that in many cases where granite has actually been erupted, the rocks which it covered have, by long exposure to its action and by being thus involved in it, been converted into its substance. This supposition is not in want of support from analogies furnished by the Trap rocks; as an instance occurs in Cantyre, as well as on the continent of Europe, where the gradual conversion of a schistose rock into porphyry, under similar circumstances, is proved in the most unquestionable manner.

In the next place, we may apply to granite as we now find it, the cases of apparently unerupted trap already examined. The immense deposits of materials which now form the alluvial tracts of the globe, the enormous masses of secondary strata which have been produced by antient materials of the same nature, all prove the magnitude of the destruction which mountains have formerly experienced, which they are now daily undergoing. Let imagination replace the plains of Hindostan on the Himálya, or rebuild the mountains which furnished the secondary strata of England, and it needs not be asked what is the extent of ruin, modern or antient. In this ruin, the highest rocks participate most largely; so largely, that we can scarcely hope to find one portion of that surface which was once most elevated above the waters. If in the progress of such extensive destruction, thus probably acting on the primary rocks at two distinct periods, every vestige of overflowing granite has dissappeared, it is assuredly an event not calculated to exite surprise.

That granite has in reality furnished a very large part of the materials of the recent strata, is proved by their constitution. Quartz, felspar, mica, and hornblende, are the chief materials of the sandstones, shales, and clays; nay, the very fragments of that rock are found every where. Even in our recent alluvial soils they abound; and it is a question worth considering, whether the granite boulders, of which the immediate origin has so often been vainly traced, are not rather the portions of decomposed conglomerate strata, or the more durable remains of the alluvial soils on which they now repose.

In an argument thus made up of probabilities and resting on analogies, it is legitimate to muster all the force that can be adduced. We have yet to learn whether, if the secondary strata were laid bare, the granite might not often be found beneath them and incumbent on the primary; protected from, at least the second period of destruction, by their covering. It may happen, for example, for any thing that we can prove to the contrary, that this very case may be present in Sutherland, where the secondary strata actually repose on granite. The observations on the junctions of granite and the primary strata are not easy to make, nor are the exposures often very perfect; so that this is one of the cases where the difficulty of proving a negative leaves conjecture alike open to all. Why may it not be added, that there is not only an absolute want of observations on this subject, but that the universality of a prejudice in favour of the aqueous origin of granite, renders some observers as unwilling or as unable to discover the truth, to explain appearances in an obvious manner, as others are, from want of experience or other causes, incapable.

This argument has hitherto proceeded on the grounds of analogy and of inference from causes and effects; but it is time to ask whether the postulate is really true, when it assumes that granite has not overflowed the strata. I have shown in another vol I 372.- work, (Western Islands,) that, in Sky, a granite, defining it by its mineralogical characters, is an integrant part of a mass of unstratified rocks which lies on a stratified limestone. If I have chosen to consider this rock as a member of the trap family, it is merely because I have thought fit to adopt the difference of age as a criterion between granite and trap, for reasons that need not here be stated. The fact, so far as this argument is concerned, remains the same; but it is plain that it will be necessary to abandon this distinction between trap and granite, whenever it shall be disputed whether the latter has ever overflowed the strata. If, in this particular instance, I had not detected the superposition of the mass, it would of course have been ranked with granite; and, in confirmation of this, we have the authority of Von Buch, a testimony never yet questioned, that granite lies above conchiferous limestone in Norway. No evidence can be less liable to suspicion than that of a person who denies the truth of the theory of which his own testimony is adduced as a proof.

We are thus at length brought to consider the further evidences that may be produced to prove the common seat of Granite and Trap, and the identity of the circumstances in which they have originated. These are deducible from similarity; in certain cases, from absolute identity of character; but I must premise, that to limit the term granite to the sole compound of quartz, mica, and felspar, is merely to abuse a mineralogical term for the purpose of evading a geological inference. This is an expedient which, however often used, is inconsistent with the rules of sound logic. In a geological sense, every rock must be considered a granite which, whatever its composition may be, forms a portion of a common mass of that rock in its most acknowledged character.

In Aberdeenshire, the leading varieties of granite are of that character which agrees with the most rigid mineralogical definition, and the superposition of gneiss over a very extensive tract of that rock can be traced with the greatest facility. But, in many places, a variety of this granite occurs, which is composed of felspar and hornblende only, passing into the ordinary kind by means of the usual fourfold compound of hornblende, quartz, mica, and felspar, and the threefold one composed of hornblende, quartz, and felspar; to both of which the term Syenite has sometimes been applied. The continuity and gradation of all these, and their inferiority to the primary strata, can be traced without the slightest difficulty. In examining more minutely the duplicate compound just mentioned, it is observed in many places to assume a fine grain, and thus at length it becomes undistinguishable from the greenstones of the trap family. But the identity does not cease even here; since, in many places, it passes, in the same uninterrupted manner, into a basalt, and, at length, into a soft claystone, with a schistose tendency on exposure, in no respect differing from those of the Trap islands of the western coast. And the same facts, precisely, occur in Guernsey; so that a mineralogist, or a superficial geologist, would, unhesitatingly, call that a basalt which is but a graduating portion of the general mass of granite.

In each case, all these rocks form a common graduating mass, and are therefore of the same date; but, in Shetland, there occurs another instance which presents, with similar features, an interesting variety of origin. In that region, there is an extensive mass of red granite of an ordinary structure, accompanied by a dark one composed of the quadruple compound, hornblende, mica, felspar, and quartz. In the latter, the same gradation into basalt may be traced, in a manner equally perfect; and that this mass is granite, is evinced by its inferiority to gneiss under all the usual well-known appearances. But it is also of a prior date to the red granite; as the latter every where penetrates it by its veins, just as it does the approximate strata. Hence its antiquity is no less unquestionable than its character.

Thus it is proved that granite, or at least a rock originating in the same causes, may possess the characters of some of the most common varieties of the most recent traps. It remains to reverse the proposition, and to adduce instances of the granitic character among these recent unstratified rocks.

[illegible] of
St Kilda
Westn Isles
II 55

If the rock of Arran be considered a granite, which it is esteemed to be, the same latitude must be extended to that of St. Kilda. Mica is often absent from both; and both contain, in certain parts, cavities in which felspar and brown quartz are crystallized

But the rock of St. Kilda is connected with common greenstone and augit rock; substances esteemed to appertain to the Trap family. If these, instead of being admitted among the traps, are referred to granite, St. Kilda will merely offer an instance to add to the former parts of this analogy.

In Sky, a quadruple compound of hornblende, felspar, mica, and quartz, passes into a triple one in which mica is absent, and at length, by a variety of gradation, into a compound of felspar and quartz, felspar and hornblende, claystone, porphyry, and greenstone. The whole mass is continuous; and, in some place or other, every member of it is found lying above the secondary strata. Here then is a mass of trap, containing a granite, undistinguishable, not only from many of the varieties which occur in Arran, but from those which are so often found beneath the primary strata.

There is little doubt that whenever adequate observers shall choose to follow the course here pointed out, and to pursue, unbiassed, that chain of observation which is only here for the first time indicated, more instances of the same nature will be brought to light, both among the traps and granites. It is especially necessary, however, to guard against being misled by mineralogical terms. It is not in the minute arrangements of a cabinet of specimens, that the great features of nature are to be studied; nor can geology ever rise to the rank of a science, if it is to be cultivated by grovelling among fragments and substituting words for knowledge.

I may now indeed add, that since this chapter has been so far arranged as to render its alteration inconvenient, I have been satisfied, by the examination, both of collections and their collectors, that the same phenomena have occurred to many observers. The case of Predazzo is one where granites of all characters form a mass with augitic greenstone; the whole being later than the red marl and the oolithe limestone; and from the observations of Von Buch, Brongniart, Boué, and others, in Norway and elsewhere, it is evident that what I had so long pointed out, required only to be more generally known to be found a very common occurrence.

I the mean time I am unable to perceive that any thing is wanting to prove the identity of origin in trap and granite. It is little likely, at least, that geology will often furnish us with evidence of a more decided nature. Nor is it an indispensable requisite to this argument, to produce numerous examples; since there are innumerable cases in science, among which this seems one, where one or two facts are as decisive as a hundred.

It is unnecessary to add feeble arguments to strong ones; but the prevailing mineral characters throughout the whole range of the unstratified rocks are of a nature to confirm that which is here proved. Yet it will not be superfluous to allude to the probable causes by which the characters of these rocks have been modified; and which have had an effect so generally steady, in distinguishing between those of the families of granite and trap, It is very probable that many of these differences arise from a constitution radically different; from the proportions of the several earths entering into them. In extreme cases, as between claystone and granite, they certainly depend on that cause. To put a hypothetical case: If it be conceived that granite was produced from the fusion of an argillaceous sandstone, and basalt from that of an argillaceous schist, the consequent difference between these two substances is easily accounted for. It is unnecessary at present to support this case by facts, as it will better come under review hereafter; but even where the composition is similar, experiment has taught us that the same mixture will, under different degrees of heat, and with a different management of it, produce different artificial rocks. Slow cooling generates a highly crystalline arrangement; quick, an obscure one. The reader may be safely allowed to make use of this analogy in his own way; as the writer himself could do little more than apply it hypothetically to the explanation of the differences between trap and granite. Only let it be recollected, that there are many differences in the conditions of the earliest and latest erupted materials, and in that of the strata among which they have intruded, which will, with a little reflection, go far towards explaining the differences in question, if the causes and effects cannot be rigidly approximated.

I will not now examine the reasons why the fluidity of granite and of trap must have been a fluidity of fusion; although, as a question common to all the unstratified rocks, it in some measure claims a place here. It will be more conveniently examined when the mineral nature of these rocks shall come under review.

Thus far this discussion has proceeded without involving considerations of a merely hypothetical nature; but it cannot be dismissed without noticing the causes which Geological Theorists have assigned for the fusion and protrusion of the unstratified rocks. To the expansive powers of a heat, or fire, situated deep beneath the surface, is attributed the enlargement or protrusion of the fluid which it has produced. The arguments shall here be stated in as brief a form as possible; as there is little to be gained by dwelling on illustrations of this nature.

It is argued that heat does operate some of the effects in question, and that it is capable of producing them all, that no other agent is known equal to fulfilling all the conditions, and that the effects are analogous to those which are witnessed in the actions of Volcanoes, whether as they relate to fusion, expansion, or elevation of the superincumbent strata. It is next argued that the existence and permanence of hot springs, and the antiquity, renewal, or existence of Volcanoes, prove that the earth is the repository of a deep seated and permanent source of heat. The questions which regard the real nature of this heat, whether it be permanent or but occasionally excited, will be more usefully discussed when the nature of Volcanoes is hereafter examined.

Of Veins of Quartz and Carbonat of Lime.

It remains yet to consider those rock veins which belong neither to the families of trap nor granite. The most abundant are those which consist of quartz and of carbonat of lime, and they will be found more analogous to mineral veins than to those which have preceded. They even throw a sort of light on some circumstances, at least, in the formation of those veins; although the greater part of that subject is still involved in impenetrable obscurity.

Veins of quartz are found in granite, gneiss, micaceous schist, and indeed in every member of the primary rocks; nor are they even excluded from the secondary, although in these they are more rare. They present many varieties of mineral character, since they are the repositories of most of the specimens of this substance which are to be found in mineral collections. An enumeration of these, forms no part of the present subject. In their dimensions, they vary exeeedingly; the breadth reaching from many yards to a thickness not exceeding that of paper, and the length, or lateral extent, varying in the same manner. As, however, they have no connexions with masses of similar rocks, their terminations in every direction can be traced, provided access be obtained to these. They are subject to ramification, and, as is elsewhere remarked, they sometimes follow the contortions of the strata in which they are contained.

The same description will serve for veins of calcareous carbonat; but it must be remarked that the rocks in which these occur, are much more limited. They are found in limestone, in serpentine, in argillaceous schist, in shale, in some sandstones, and in trap; but are extremely rare in micaceous schist, or the other rocks of the primary division: in granite and gneiss, they have never, I believe, been observed.

The origin of these two kinds of veins has been a matter of dispute among different theorists. They have been called contemporaneous by one party, and supposed to be produced by the same unaccountable crystallization that formed the including rocks. It will be time enough to examine the validity of this theory when nature shall establish new laws of crystallization, and when the past and the future are alike present. Those who have favoured the theory of an igneous origin or consolidation, have imagined that quartz and carbonat of lime were secreted in a state of fusion, into fissures formed by the shrinking of the indurated strata. Nothing can be much more gratuitous than this supposition; while, even if admitted, it would not explain the formation of these veins in granite and trap. There appears little difficulty in tracing them to a watery and gradual infiltration of the two minerals in question; whatever objections may be urged against the production of extensive veins of quartz in this manner. The process may, in fact, be traced to a sufficient extent, to allow us to infer the possibility of all that is here asked.

The infiltration of quartz and of carbonat of lime through rocks, is proved by the formation of chalcedonies, quartz crystals, and calcareous spar, in the inflated cavities of trap rocks; and in these, every stage of the progress can be traced, as I have fully shown in former writings. It is proved in the case of calcareous spar, by the very common formation of ordinary calcareous stalactites. It is proved that quartz crystallizes from water in these very veins; because that variety of rock crystal which is formed, like nitre, not by the process of gradual increment on one central nucleus, but by a process commencing in various distant parts at once, often contains water. Lastly, I have traced the actual formation of these veins in primary limestone in Glen Tilt; where the narrower parts of the fissures were filled by a consolidated spar, and the sides of the remainder were covered by an incipient crystallization; the vacancy containing a solution of carbonat of lime. The end of the process can, in such a case, be easily predicted; and there is no difficulty in applying the same explanation to every analogous instance. I ought to add, in terminating this subject, that the same vein sometimes contains both quartz and calcareous carbonat;

a case exactly similar to that of the siliceo-calcareous nodules found in trap.

Veins of felspar and of compact felspar are not uncommon in those rocks which contain veins of granite and porphyry. It would be superfluous to describe them particularly, as they differ in no respect in their features from those of granite and trap. It will be found that some of the veins of common felspar are varieties of granite veins, as is frequently indicated by occasional crystals of imbedded quartz, and that the others are the bases of porphyries in which the usual imbedded crystals are rare or altogether wanting.

It is barely necessary here to mention the existence of pitchstone veins; since, as it seems to be the only form in which that rock occurs, it will be better to examine the whole subject in its proper place.

The last species of veins requiring notice, are those conglomerate veins which cannot be ranked with the mineral veins described in a subsequent chapter. They are probably very rare, as I have met with only three instances of them. In these, the materials consist of various rounded stones with clay and sand, cemented in the usual manner of conglomerate rocks; and they have evidently been formed by the casual falling of the loose materials into open fissures.

CHAP. XI.

On the concretionary and crystalline Structures of Rocks.

The internal structure forms an important part of the natural history of rocks, and is also interesting, from the hints which it may afford respecting their formation, and from the errors to which it may give rise. The modifications of the concretionary structure may be divided into the large and the small, but the limit is undefinable.

Of the laminar, foliated, and schistose Structures.

The most important, perhaps, if not the most conspicuous division of the large structure, is that to which the very wide term of laminar may be applied. This is that modification which has so often been confounded, under some of its forms, with the stratified disposition; giving rise, in the cases of Trap and Granite, to serious errors. One of the most interesting varieties in this division, is that which occurs in granite. The size of the concretions, if such they are to be considered, is often immense; while, for a certain extent, they sometimes put on the appearance of strata so accurately, that it is not very surprising if they have misled incautious observers. It is not often, however, that the laminar form is so perfect; for, on a careful examination, it will generally be found that the sides of a lamina are far from parallel, and that they speedily disappear in their progress, being irregularly entangled and implicated with others, not only of different sizes, but of various irregular forms. It is not unfrequent for these laminæ to be curved, so as to have a convexity and a concavity; while, in other cases, all their boundaries are convex, causing the laminar to approximate at length to a large spheroidal structure. Further, they pass into the cuboidal or square prismatic structure, in consequence of fissures at right angles to their planes; and, in the same manner, they are sometimes split into imperfect columnar divisions.

The minuteness of the laminar structure is at times such, that granite possessing this character has been called schistose; but the difficulty which attends some cases of this nature is examined in the chapter on the Destruction of Rocks. (Chap. xiii.) It is proper here to add, that the larger laminar structure is most frequent in granite; but that it occurs in some of the trap rocks, including porphyries, and is, in particular, very conspicuous in hypersthene rock. The smaller laminæ are found principally in the traps and in pitchstones: and it thus appears that this structure is nearly peculiar to the unstratified rocks. It occasionally happens that the laminar structure is to be discovered only after exposure to the air, a circumstance necessarily noticed in the chapter on decomposition, and that it may be combined with other varieties, as with the columnar, in many of the trap family. It is also found in the veins of the later traps and the antient porphyries, as well as in the products of volcanoes.

The circumstances thus detailed respecting the rocks to which this structure belongs, added to a careful and unprejudiced eye, must be the Geologist's guide in distinguishing laminæ from strata, a concretionary form from a real stratification.

The foliated structure is distinguished from that properly called laminar, by an undefined, or else a comparatively unlimited divisibility; and the examples of it are found in the argillaceous schists, in the micaceous schists, in gneiss, and in others of the analogous primary rocks. It is conveniently divided into the foliated strictly speaking, and into the schistose.

In the former, which occurs in the primary rocks that contain mica, the divisibility is the result of the position of the mineral; and that position, it is elsewhere shown, may be the consequence, either of deposition or of crystalline polarity. It is unnecessary to dwell on the varieties of aspect which this structure presents; but these will be found to consist, as in gneiss, in its irregularity and imperfection; or, as in the finest and flattest chlorite schists, in its extreme tenuity and flatness. The analogous structures which occur in the secondary calcareous or arenaceous strata have evidently resulted from the mode of mechanical deposition by which these have been produced; and, very generally, from the conspicuous interposition of very slender portions of clay or of mica. These belong properly to stratification.

The schistose structure is one of those which may truly be called concretionary; as it occurs in a homogeneous rock, and is independent of stratification. It is almost limited to the argillaceous schists; yet not necessarily to those which are homogeneous, as the mixture of sand, gravel, or fragments, does not prevent its existing in the simpler base by which these are united. A similar structure occurs in the sandstone of Sky, and it will probably hereafter be found in other instances where it has been little expected; in which case even the secondary strata may often possess a truly schistose or laminar structure, where the appearances have been attributed to stratification.

The schistose concretionary structure is not necessarily straight, but is sometimes found to be curved, as in clay slate: and that the curvature belongs to the structure and not to the bed, is evinced by the regularity or evenness of the latter. It is possible that this circumstance may tend to explain some of the complicated curvatures that occur in beds of micaceous schist under similar circumstances; but the suggestion was reserved for this place, as it seemed that its value would here be better understood. But that all curvature is not of a concretionary origin, is proved by the remarkable fact noticed in the ninth chapter as occcurring in the schist in Plymouth Dock-yard; where the contortions are marked by differences of colour, and the schistose structure is at angles to them.

It is not unusual, in the argillaceous schists, for the observer to mistake the direction of the schistose arrangement for the plane of the bed. It is true that these are sometimes coincident, as in the secondary schists; but, in the antient schists, the former is also, perhaps most generally, at angles, often very considerable, to the plane of stratification. The mode of making this distinction is stated under the head of argillaceous schist.

Of the prismatic and columnar Structures.

The phenomena of decomposition might seem to throw some doubt on the existence of any prismatic structure different from the columnar, which is commonly considered as forming a separate division. It is common and well known in granite, and it also occurs in some rocks of the trap family. It is found almost invariably on the large scale, and is possibly, in these cases, only a modification of the laminar structure, produced by fissures. Where it occurs in the sandstones, it appears to be more certainly referable to this cause. When it was thought certain that the spheroidal exfoliation of the cuboids of granite was a proof of a spheroidal concretionary structure, it was natural to consider these as prismatic concretions. That will still be true should this be proved; but as some of the cases of this nature are unquestionably shown, in the chapter on the destruction of rocks, to arise from the action of the atmosphere, the whole question must remain open for further inquiry.

The columnar structure, on account of its symmetry and artificial appearance, is unquestionably the most interesting of all these modifications: an interest not a little enhanced by the difficulty of explaining its origin. It presents many varieties, and occurs in rocks of very different characters.

The most remarkable forms of this nature are those which exist in the rocks of the trap family; in the history of which will be found the names of the particular substances in which they occur, together with such minuter details as are not necessary for the present purpose. (Chap. xxxix.) These columns are of various sizes, ranging from the diameter of even an inch to one of many feet, and, in height, from a foot to many hundred feet. They are almost invariably associated in groups, so as to occupy the whole, or portions, of the stratiform beds occasionally found in the trap rocks. In these cases they are generally parallel, with more or less of exactness; but, in some, they are variously and irregularly implicated. Occasionally, they are even intermixed with amorphous matter of the same nature. They are, commonly, more or less accurately vertical; because the beds which they divide in a perpendicular manner, are, in the same way, more or less strictly horizontal; but they are also occasionally curved in a variety of modes. They are, further, often divided by transverse joints, of various forms, though sometimes simple. The angles of these columns vary in number, yet so that the prevalent forms lie between the four and sevensided figures; but it is essentially necessary to remark, that the contact is always perfect; neither vacuity among the angles, nor interval between the approximate sides intervening. The more imperfect forms of this description gradually pass into an irregular prismatic structure; and that at length becomes so indefinite as to be confounded with a mere tendency in the solid rock to a vertical fracture.

When these columnar traps are subject to decomposition, it is sometimes observed that they desquamate in successive crusts, so that a spheroidal nucleus at last remains where there was once a prismatic joint. This has been supposed a proof of a peculiar concretionary structure giving rise to the prismatic form, the arguments respecting which will be immediately considered.

As connected with the trap rocks in their general characters, it is proper here to observe that some lavas occasionally assume the same figures. It has sometimes been said that this occurrence took place only where such lavas came into contact with the sea in the course of their progress; and it has been argued that a similar cause may have produced the columnar form in the trap rocks. But the assertion is unfounded, in all respects; inasmuch as columnar lavas are found where no water can have been present, and amorphous ones occur beneath the sea.

If the columnar structure is common, in sandstones, it must have been overlooked by geologists; and the only two instances with which I am acquainted are found in the island of Rum and at Dunbar, in Scotland. The fact is noticed in the account of that rock; (Chap. xxxv.) the inferences are more usefully placed here.

The columns that occur in the sandstone of Rum are of small dimensions, not exceeding a few inches in diameter. They lie in the stratum, in perfect contact, presenting the usual intermixture of polygonal forms; and, what is especially necessary to notice, they are covered by a mass of basalt. At Dunbar, the sandstone in which the columnar arrangement is found, is that which is known to be the lowest of the secondary strata, and which, throughout a great extent of country, presents only the usual stratified character. The columns are limited to a small space, but are of considerable dimensions; attaining two feet or more in diameter, and a length of 15 feet or upwards. Where this columnar structure occurs, the character of the rock is changed in a greater or less degree; becoming more compact, harder, and, in some places, passing into a perfect but coarse jasper. In addition to this, it presents the indications of an internal concretionary structure, similar to that which might be inferred to exist in the columns of trap, from the mode, already mentioned, in which they are found to desquamate. The transverse sections of each prism are marked by concentric lines of different colours, whitish and reddish; which conform accurately to the sides and angles, towards the exterior, but become gradually curved as they approach the centre; indicating the probable existence of a spheroidal nucleus. This disposition, it is plain, is real, and unconnected with any agency of the atmosphere.

The columnar shales or argillaceous iron stones as they have been called, seem to be in every respect analogous, and to admit of the same reasoning. This modification occurs on the large scale in Arran, as well as in Orkney; the prisms being of large diameters, but divisible by transverse joints into very flat tables, and marked by other peculiarities described in the account of this substance. (Chap. xxxviii.)

In trying to explain the origin of this structure in the last named rocks, it is first to be remarked, that the appearance is limited to a small portion of extensive beds which elsewhere preserve their natural characters; and that, in both, particularly in the sandstone, there is a simultaneous change of the mineral character of the rock. The sandstone passes into jasper; that being evidently the case only, where, from being intermixed with clay, and thus approaching to shale, it is of a compound nature. The simple strata that are found in it are indurated; and the purer sandstone is also hardened, so as to resemble some of the varieties of quartz rock. These are precisely the changes that take place in similar sandstones where they are found in contact with trap rocks; appearances so well known that it is unnecessary to name any examples except that in Salisbury Craig near Edinburgh, and that described by myself at Stirling castle: (Geol. Trans.)

It is well known that the masses of trap incumbent on the upper strata, are often so entirely removed as to leave no traces of their existence; and near Dunbar, there are numerous detached portions of these rocks, which have probably been once connected into a continuous mass. It is not therefore unreasonable to suppose that such a mass may have once covered the portion of this sandstone which has undergone that change to jasper which, in other cases, these are known to produce. The following facts seem to prove that the columnar structure was the consequence of the same action.

In Rum, the columnar sandstone actually lies beneath a mass of trap; so that the fact of their simultaneous presence, at least, is proved. This, it is true, is as yet a solitary instance; but here, fortunately, direct experiment comes in aid of the supposition that the action of heat has produced the columnar structure of sandstone. The sandstones used for the hearths of iron-furnaces, after long exposure to the heat of these, become divided into polygonal prisms, exactly resembling those of the natural prismatic sandstones, on a small scale. In this case, there is no shrinking, as in dried clay, to account for the appearance; the sides remaining in perfect contact, just as they are found to do in the columnar traps. The same circumstance occurs in those sandstones which are heated for the purpose of making roads in Derbyshire. Here therefore it is directly proved, that heat is capable of inducing the prismatic structure in a solid sandstone; and, that this is not the developement of an original concretionary structure, is proved by the fact, that in the hearthstones which have undergone this change, the arrangements of the prisms is always vertical to the plane of the stone; a remarkable analogy to their mode of arrangement in the trap rocks.

Ignorant as we are of the nature of the concretionary structure, it is still certain that it bears a kind of analogy to crystallization; and experiment proves that this arrangement, if it be not rather a concretionary one, may take place in rocks, without fluidity. It is also remarked in the chapter on curvatures, that a curved structure is sometimes developed in rocks by heat. The present discussion may perhaps render it doubtful whether this is not rather the generation of a concretionary structure. It is impossible to pursue this argument further, for want of a greater store of facts. It must be left to make that impression which is all that an imperfected train of reasoning is entitled to expect. Yet, in terminating these observations, it is right to remark, that the decided union of the concentric arrangement with the prismatic form in the sandstone of Dunbar, renders it probable that this arrangement exists also in the prisms of trap; invisible, from want of contrasts of colour or texture, and developed only on wasting.

It remains to inquire whether this fact may not be analogically extended to account for the columnar form of the trap rocks. Different causes have been assigned for this by geologists. It has been supposed to result from the division of a mass of a soft and moist material, by drying and consequent shrinking; and it has been attributed to crystallization, from a state of igneous fluidity, or from solution in water. It is useless to examine that theory which conceives that it arose from the contact of fluid trap with water. That would scarcely explain its nature, even were this a fact proved; which, as I have already remarked, it is not.

There is no resemblance between the prisms of trap and those formed by the shrinking of clay; the essential difference lies in the absolute contact of the former; and that objection is insurmountable. To call the arrangement of a basaltic prism crystallization, is, on the other hand, entirely to lose sight of the true nature of this mode of arrangement, which consists in the production of definite geometrical figures by the repeated addition of particles of a definite form, whether these be simple atoms or compounded chemical molecules. In the prisms of trap, the laws of geometry and chemistry are equally violated; and the objection applies equally to both modes of crystallization, whether from solution or fusion.

On the other hand it appears, that sandstones exposed to heat do assume the prismatic form, while it is certain that the trap rocks must have often retained their heat long after they had lost their fluidity. It is unnecessary to draw out the argument further. The prismatic form might have occurred even after the rock was consolidated: if any additional facility is gained by the supposition, this change may be conceived to have gradually taken place while a state of tenacity still permitted a certain degree of motion among the parts.

A small and irregular prismatic disposition is sometimes found in the pitchstones, as well as among the traps; and it can scarcely be considered as more than a modification of the laminar form into which it passes. In certain argillaceous ironstones and jaspers, there has also been observed a prismatic arrangement on a small scale; which is further often singularly marked by protuberant joints, or by small stripes or channels parallel to the prisms. A similar disposition exists in that substance, called madreporite limestone, from its resemblance to an organic structure. Respecting these, there is nothing further known, from which an explanation of the causes of these arrangements can be derived.

There is yet one modification of the prismatic structure remaining, which requires notice; on account of the misapprehensions which have been entertained respecting its cause, and from its misapplication toward the support of the views of certain speculating geologists. This relates to the iron stones known by the name of septaria, which consist of spheroids, generally uniform on the outside, but divided within into polygonal figures, of which the intervals are filled by calcareous spar. It was supposed, that these stones had experienced the influence of fire, and that, in the act of consolidation, the calcareous matter had been separated from the compound mass; it having been conceived impossible that it could have entered from without. But the solution of this difficulty is exceedingly simple; and the occurrence is an obvious instance of the shrinking of a mass of moist earth. In some of the septaria, the external surface is not solid, but the prisms reach it; and, in these cases, the ease with which carbonat of lime might have entered into the intervals is evident. Where the surface, on the contrary, is unbroken, it is no less easy to understand how, during the drying of such a nodule of clay, that part would first consolidate; while the interior would necessarily shrink and split, from the dissipation of the water through a substance unquestionably capable of permitting its transudation. The subsequent infiltration of lime into the cavities .thus formed, is not only easy to apprehend, but is a fact of daily occurrence in rocks of a far more compact nature, namely in the traps; the amygdaloidal cavities of which are filled in the same manner. The resemblance of this process to that which takes place in the ammonites containing calcareous spar, is abundantly obvious.

Of the spheroidal Structure.

The spheroidal structure is found under different modifications; some of which are among the most inexplicable phenomena of this nature which geology presents. The explanation of those which approach in their nature to crystallization, is not so difficult; and these examples serve, in some measure, to connect two processes, otherwise very different in their natures. The large spheroidal structure of granite, already mentioned, cannot with propriety be ranked with this; nor that which occurs in Trap, in Rum, and elsewhere.

In the secondary sandstones of Egg and other places, there are found large spheroids imbedded in the ordinary strata. These are distinguished by a greater hardness of texture than the surrounding rock, whence they are easily separated as it wastes away. Their own texture is also unequal between the centre and circumference; and it not unfrequently happens that the superficies is cracked into polygons. How far the influence of Trap may have tended to the production of these, must be conjectured from the circumstances respecting the prismatic structures of sandstone formerly stated, and from the fact that these spheroidal sandstones also occur in the vicinity of trap. I may here add, that concretions of large size have lately been brought from the new discovered land of South Shetland, consisting of the halves of very flattened spheroids; as if such figures had been cut through according to their equatorial diameters by a sharp tool.

In the argillaceous limestone, as well as in the accompanying sandstones of Sky, highly flattened spheroids of large dimensions are found attached in pairs by a cylindrical stem, and imbedded in the surrounding rock; from which they are easily separated after its destruction, on account of their superior hardness. They bear no resemblance to organic forms; and although they have also been observed in other parts of Europe, and in other limestones, no explanation of their origin has been suggested. These also occur in the vicinity of trap rocks, in my own experience at least.

The smaller kinds of spheroidal structure are more numerous, and present greater variety. In the siliceous schist of the Shiant isles and Scalpa, it is ascertained by decomposition, that the internal structure consists of small aggregated spheroids; the intervals of which, being of a different nature, become converted into clay on exposure, leaving a botryoidal surface. In the fresh rock, this cannot be suspected. The softer shales of the former islands are also frequently found to consist of an aggregation of spherules not larger than mustard seed. In these cases also, trap is present; and it is easily proved that the rocks in question were once the ordinary shales of the coal strata, which, in undergoing induration, have also experienced this change of structure. Where some of the claystones of Arran are invaded by trap veins, they assume, in some places, an imperfect spheroidal tendency; which gradually becomes more perfect where they approximate to the trap; while their substance, at the same time, is converted into an anomalous stone resembling those cherts which have been sometimes called hornstone. An inequality of the internal texture is here also ascertained, by the botryoidal surface which these assume on exposure to the sea.

It is now important to remark, that these last-named spherules, wherever the forms are most perfect, present a concretionary structure passing into one which is decidedly crystalline. Brilliant fibres radiate from the centre, and are repeated at intervals so as to form successive concentric crusts of the same nature; or else these crystalline spheres are surrounded with crusts in which no fibrous structure can be traced. There is thus a transition from the most perfect crystalline to the most imperfect concretionary spherule.

In attempting to explain these appearances, it is interesting and important to observe how these spheroidal crystallized forms coincide with those which occur in melted glass under certain circumstances, and how accurately they resemble the analogous appearances produced in Mr. Watt's well known experiments, where the arrangement was produced after the fused trap had lost its fluidity. Thus it is equally easy to comprehend how a solid mass of any of the above named rocks, softened, if it is necessary to suppose so, without fusion, or otherwise under the long continued influence of heat, might have assumed a similar species of structure. As also, in one of these cases, there is a gradual progress from the most perfect crystalline to the most imperfect concretionary arrangement, there can be no reason to doubt, that in every case, the latter may also be produced by the same causes. It should here lastly be added, in confirmation of this theory, that a spheroidal structure of a similar nature exists in the Trap of the Shiant isles. Those of the granite, the porphyry, and the greenstone, as it is called, of Corsica, will naturally occur to every geologist.

A spheroidal structure, terminating also, by wasting, into botryoidal forms, has been observed in certain limestones, as in that of Sunderland. A similar arrangement is occasionally found in the sandstones; and sometimes, in the red varieties, it is indicated by the presence of white spheroidal spots, or portions. Of these, no explanation has been suggested, and I have none to offer.

The spheroidal structure of the oolithe limestones of England, appears to be merely an aggregation of rounded grains, and requires no notice. That of the pisolithes, which consists of crustaceous agglutinated spherules, is probably the result of a deposition from water; the exact nature of which is not very apparent. I shall here forbear any remarks on the spheroidal structure of pearlstone, as it will be noticed in its proper place hereafter. (Chap. xl.)

Of the venous, cavernous, fibrous and scaly Structures.

In many rocks, it may be observed, that where the surfaces have been exposed to the weather, they present a reticulated appearance, as if from the intersection of veins, of a nature harder than the general mass of the rock. On breaking such rocks however, no corresponding appearances are found in the interior; the whole mass presenting an uniform texture and colour. This peculiarity is very frequent in granite; but it occurs also in gneiss, in micaceous schist, and in the sandstones. It has been conceived to arise from some original structure, but is, at best, a very obscure circumstance. It deserves notice, perhaps principally, because it has been used as an argument to prove that all veins are of similar origin, or, in other words, that, in the ordinary acceptation of the term, no such thing as a vein exists. The analogy is clearly one of those superficial resemblances calculated to operate only on minds of a similar structure; while, if there is any one fact in geology that is beyond the regions of dispute, it is that of the posteriority of veins to the substances which they traverse.

A cavernous structure, sometimes rendered visible in sandstones by decomposition, may almost be considered as a variety of this; since the separation of the cells may be considered as formed by such durable intersecting laminæ. The appearances which attend some of these cavernous and reticulating structures, are often very singular; but as they are discovered only by decomposition, they are more particularly noticed in the chapter on that subject. (Chap. xiii.) That they depend on some internal arrangement produced subsequently to the deposition of the strata, can admit of no doubt; but, respecting the nature of this, we must as yet confess our ignorance.

The fibrous structure is the last which can strictly be enumerated among the concretionary modifications; and it seems to unite them with those which are properly of a crystalline nature. It is known to occur in the carbonates of lime, as in the satin spar and in the limestones of Egg. In the former, it is more decidedly crystalline than in the latter, resembling the corresponding arrangement so frequent in gypsum. It is also not very uncommon in the argillaceous schists; in which, as these are not susceptible of the crystalline arrangement, it must necessarily be referred to the concretionary structure. As to other fibrous arrangements seen in rocks, including that which has been called bladed, they are purely crystalline; their peculiar aspect being produced by the lengthened forms and parallel arrangements of the crystals.

Of the scaly structure, it is sufficient to say that it is one of those which, when it occurs in rocks of a crystalline character, must be considered as among the first in the order of crystalline arrangements. As a consequence of the mechanical deposition of flat particles or scales, it requires no notice in this place.

Of the porphyritic, granular, and amygdaloidal Structures.

The structure called porphyritic is purely crystalline, and is that which confers the peculiar character on the porphyries. It is by no means however deficient in interest; as it is known only in those rocks which, from many circumstances, are proved to derive their origin from fusion. When indeed we consider, that in this case, a single crystal of a perfect form is surrounded by an uncrystallized mass, it offers in itself a proof of the species of fluidity under which the whole must have been consolidated. No imagination can assign an expedient for producing this effect from a watery solution; while the existence of the porphyritic structure in volcanic rocks, affords every proof of the nature of its origin which can be desired.

The granular structures which belong to the sandstones and conglomerates, being purely mechanical, need not be noticed; but that of granite and the analogous rocks, being of a crystalline nature, is here deserving of regard. It has been maintained that this structure has been the produce of watery solution, by those who still chuse to consider granite of aqueous origin. The argument, as far as its texture or structure is concerned, belongs properly to this place.

Granting the greatest facilities to the preceding supposition, by admitting the solution, in water, of earths, noted for the extremely limited degree in which they possess this property, and granting, still further, that they were able, under these circumstances, to enter into all the multifarious combinations which are to produce quartz, felspar, mica, hornblende, and many other minerals, it remains to invent a new process in the chemistry of crystallization, by which all these combinations should have been in an instant deposited together in a solid mass. If a successive deposition of the different minerals be conceived, it is impossible to explain the mutual interference which takes place among them, and which characterizes the crystalline granular structure. The imagination that would produce such an effect from such causes, must not be allowed to flit about vague generalities, but is bound to contemplate steadily every minute circumstance implied in such a process.

But nature and art both are ready to prove that this effect takes place without difficulty from fusion. The glasses of our furnaces separate into various mineral compounds on cooling. The same results take place from the cooling of fused basalts, where the previous combinations have all been dissolved by one general fluidity. In the trap rocks, the granitic structure is common; and these, it is granted, are the products of fusion. The lavas of volcanoes, if it could be necessary to insist on facts so well known, are in a state of liquid fusion, in which every integrant earth is left free to enter into such combinations as the infinite complication of affinities may direct. If these are cooled suddenly, they are arrested before they can enter into new compounds, and an uniform rock, or sometimes a glass, is the result. If, on the contrary, sufficient time be granted, the consequence is the generation of numerous minerals, producing, not only the granitic structure, but the porphyritic also. It is not necessary here to argue the question of graphic granite, which was originally brought forward to prove the same conclusion; since the basis of the reasoning is the same, and the nature of that structure is particularly described under the head of granite. (Chap. xxiv.)

The last structure to be noticed is the amygdaloidal, and it is convenient to examine it here, that the whole of the subject of structure, as far as it forms an object of geological theory, should be seen in one condensed view. If the explanation of its cause here given be admitted, it will however be seen that it has no proper title to rank among the modifications of the concretionary structure.

This structure is limited to the trap family and to the volcanic rocks. It is universally admitted, that the cells of volcanic scoria have been produced by aëriform matters disengaged during the process of fusion. Similar cells are found in the trap rocks, as I have elsewhere shown; (Chap. xxxix.) and these rocks have also been produced in the same manner.

Now the cells which, in either of these classes of rock, contain the amygdaloidal minerals, differ in no respect in form and disposition from those which are empty; and if their internal surfaces be examined, it will be found that they are often coated with a similar vitreous varnish. These cavities are not always filled with the minerals which they contain; but present vacuities, in which the crystalline terminations of the minerals are often defined. In the next place, two minerals, or even more, are sometimes found in one cavity; in some cases interfering with each other's forms. Lastly, similar cavities occur in the same rocks, sometimes of considerable size, yet connected by a gradation of magnitude with the smaller cells. These seem to be the circumstances most essential to the argument under review.

Partly perhaps from the existence of amygdaloidal nodules in volcanic rocks, and partly from a supposed necessity for thinking that every mineral contained in a trap rock must necessarily be, like its base, of igneous origin, it has been argued by those who speculated more than they observed, and reasoned ill from what they saw, that these minerals also were the produce of fusion, and that they had been secreted during the cooling of the rock, so as to form the cavities which they occupy. I need not state the various minute details, sometimes neither very intelligible nor very requisite, by which this opinion was supported. The igneous theory of trap would be feeble indeed, had it no firmer foundation than this to rest on; while the notion of such a chemical secretion is, to say the least of it, inconsistent with all our chemical experience.

It is quite intelligible, that crystals of any mineral should be formed in a fluid mass of the earths, as they are in porphyries and in many volcanic products, during the very process of consolidation; but it is not to be explained how they should in this manner form rounded nodules; still less, how the cavities which include them should ever be partially empty, or present the peculiar surface already described. The vacant spaces must have contained an elastic fluid; and when we find that these vacancies are similar in their forms and surfaces to the cavities which are entirely filled, and to those which are utterly empty, it is a fair conclusion that the whole, alike, owe their origin to inflation. It is then into previous cavities that the minerals of the amygdaloids have been deposited; and it only remains to inquire whether this has been effected during the igneous condition of the rock, or from posterior infiltrations of a watery solution of earths. It must not here be objected, that the larger cavities could not have been produced by inflation; for it is in those, more particularly, that the proofs of watery infiltration are most satisfactory.

I have shown, in the account of the Western Islands and elsewhere, that stalactites of chalcedony were often found to depend from the upper parts of such cavities, partly filling the vacuity. In other cases, the stalactite is found to correspond with an inferior stalagmite; offering a case precisely resembling that which occurs in the ordinary calcareous stalactites of caverns. Lastly, the superior dependent stalactite is more or less perfectly imbedded in a laminar chalcedony, rising from the bottom of the cavity which it is at last destined to fill, and thus to form a solid nodule. If any appearances can prove a watery infiltration of siliceous matter, these are of that nature. In other instances, the siliceous stalactite is involved in calcareous spar, which, as in the former case, either leaves an empty space or fills the whole; forming a compound amygdaloidal nodule. Here, it is evident that the calcareous spar is posterior to the stalactite; and thus also a watery infiltration of two minerals into one cavity is proved.

It is easy to extend this reasoning to the ordinary case of the concentric agate nodules, which may or may not contain calcareous spar. In these cases, the siliceous matter has been deposited by a more gradual infiltration over the whole of the surface of the air-vesicle; producing the concentric appearance of the coats, in consequence of the successive deposition of a material differing in texture or colour. If the agate contains a central portion of calcareous spar, it is obviously only a variation of the former case. It is thus also easy to explain, why the agate sometimes contains an interior covering of siliceous crystals, from changes that have taken place in the quality of the solution; these presenting their usual geometric forms, or else being confused, accordingly as the cavity is filled or not.

It cannot be objected that siliceous earth is insoluble in water; because its solubility is proved by numerous facts, and by none more decidedly than the existence of vegetable remains in chalcedony. And that the solid substances in question can transmit water, is certain; since it exists in rocks, and finds a passage through many much more solid than the amygdaloidal bases, as is proved by the daily formation of calcareous stalactites. I have also proved, that the agates are sufficiently porous to transmit oil, and also sulphuric acid; that property being the basis of the process used for staining them black. There is therefore no difficulty in understanding, how the rocks should admit the mineral solutions into their cavities, and how the first crust of agate should permit the deposition, not only of successive ones of the same nature, but, from changes in the nature of the solution, of calcareous spar also.

One source for the amygdaloidal nodules is thus established, but it does not follow that this is the sole one. The minerals which these cavities contain are numerous and various, and we have no proof that some of them can be formed by aqueous deposition; while it is certain that they are sometimes produced from fusion, as they are found constituting imbedded parts of the volcanic rocks. I have shown (Geol. Trans.) that silica can be sublimed by heat; and the same fact has been affirmed to occur in the volcanic products of Vesuvius, by observers whose testimony cannot be questioned. It is possible that compound minerals may be subject to the same laws; and it is also perfectly intelligible, how in a fluid or tenacious rock containing the cavities produced by inflation, those minerals which have sometimes crystallized in the general mass, should have also protruded themselves into the cavities.

by [illegible]
Faraday
[illegible]
[illegible] vol 13.155

There are probably, or possibly, therefore, two origins to be assigned to the amygdaloidal nodules, both of the trap rocks and the volcanic products; however the mode of explaining the igneous method may here differ from that which has been adopted by those who were more anxious to believe than able to explain. Admitting them both, the question respecting the igneous origin of the amygdaloidal bases of the trap rocks, rests precisely on the same foundation as before; as the essential circumstance consists, not in the presence of the nodule, but in the formation of the cavity which contains it.

Of the Nature of the concretionary Structure.

It remains to see if any light can be thrown on the general nature of this mysterious process. That it differs essentially from crystallization, was already noticed. It is not concerned, either in the disposition of original and similar molecules, or in arranging them into geometric forms. Yet its phenomena bespeak a tendency in the particles, or finer fragments constituting stones, to arrange themselves by a predominant attraction, into certain forms rather than others; however irregular, or uninfluenced by geometrical rules, these may be. A simple and obvious instance of this tendency may be seen in the disposition assumed by fine powders or sand under water, where these are free to move. That it exists in bodies fluid from fusion, is proved by the appearances that occur in the slow cooling of liquid basalts artificially fused. Lastly, that it may happen in solid bodies, is proved by the phenomena which take place in heated sandstones,. in trap after it has ceased to be fluid, and in solid glass; which undergoes a change of internal crystallization from changes of temperature, and even effloresces, as in the achromatic object-glasses of telescopes. In a series of experiments instituted for the same purpose, I have also proved that every metal can completely change its crystalline arrangements while solid, and many of them at very low temperatures. In fact the power of motion in the particles of solid bodies, is proved by their changes of dimension on alterations of temperature; and it is not therefore extraordinary, that in those which have the properties of crystallizing, a tendency to their peculiar crystalline forms should occur. It is also not surprising if, being thus in motion, they should assume other and less regular forms, as they do from the fluid state.

We have no right to assume that the parts of such matter may not have the power, by mutual attraction, of assuming forms which are not geometrical, even though they should be heterogeneous and shapeless; knowing nothing of the nature and laws of that force by which similar and definite molecules affect geometrical forms. The limit between crystalline and mechanical attraction may be undefined, and so may the resulting forms. Thus the concretionary structure may bear a real analogy to crystallization, or it may even be supposed a modification of that process. We know that it exists; we are ignorant alike of the laws of both. But that they have a real connexion, is proved by the phenomena above recited respecting the smaller spheroidal structures. In these, it is absolutely impossible to define the point at which the one ceases and the other commences. The radiated crystalline spherule passes into one consisting of solid unradiated concentric crusts; and that again, in a manner equally gradual, into a solid sphere without any internal structure.

I know not, that at present, any further light can be thrown on this obscure subject. As far as relates to the magnitude of some of the masses considered as concretionary, there is no cause for objections. We can even see no reason why nature might not have produced a crystal of mountainous bulk, provided the requisite circumstances were present. The polar tendency of crystallization is often prolonged through various obstacles, as is daily seen in minerals: it may be protracted indefinitely along the atoms of a compound mass, as is evinced by the granite vein in Coll elsewhere described; (Western Islands.) The tendency to form certain concretions may equally be unlimited; and thus it needs excite no surprise, if even the granitic laminæ of the Alps, which have been supposed the products of an extensive but disturbed stratification, have been produced by a concretionary arrangement analogous to crystallization.

Polarity of Crystals

CHAP. XII.

On the Origin, Materials, Composition, and Analogies, of Rocks.

If it is the first error of the observer to see, like the miner, but a very limited number of rocks in the system of nature, it is not long before he falls into one the very reverse; creating for himself permanent distinctions from every incidental variety which comes under his notice. Time, however, speedily corrects this error, and teaches him, that however the aspects of rocks may be multiplied, Nature has limited these productions by a very confined set of general and constant characters.

Of the Constituents of Rocks.

A small number only of the Earths which Chemistry has discovered, forms the materials of all the rocks; united, in some cases, with alkalies and with certain metallic oxydes. In some, a single earth is found; in others, two or more exist; and these are either mechanically mixed, or united by the laws of chemical affinity. Thus are formed those rocks which are considered simple; simplicity, as applied to rocks, meaning simplicity of aspect. Limestone presents an example of a rock, in every respect simple; while basalts and clay slates, though simple as rocks, are chemical compounds or mechanical mixtures.

Besides these distinctions, the earths are sometimes formed into separate minute bodies, or minerals, which are again united so as to constitute rocks; and these may be, in themselves, either simple or compound minerals. Sandstone offers an example of a simple rock of this kind; simple in its chemical nature, but an aggregate as to its general character. Hornblende rock is an example of an analogous aggregate, but one in which the integrant minerals are chemical compounds. But there are differences here, even in the mode of aggregation; which, in some cases, result from the chemical interference of a simultaneous crystallization, in others, from the mere mechanical approximation of the parts, and lastly, from the union of those two processes. Granular limestone is an example of the first; and instances of the last are to be found in different varieties of sandstone.

In compound rocks, different kinds of minerals are visibly united into a common mass; which thus presents a sort of uniformity throughout the whole, however the separate parts may differ. Such compounds may consist of two or more minerals; and, within certain limits, they seem to be ruled by laws as general as the simpler rocks. These compounded rocks vary, like the former, in being purely crystalline, or otherwise; and as granite presents a familiar example of the first, so quartz rock, and some of the compound argillaceous schists, afford instances of the other two.

There is still another description of compound rocks, to which the term conglomerate has been applied. In these, not only different minerals are united in a mechanical, a mixed, or a chemical manner, but fragments of former rocks, either simple or compound, also enter into their structure. Such fragments vary in size, from the most minute visible particles to others of many pounds weight, or even hundreds; and these rocks offer, in consequence, numerous varieties which are fully treated of in the author's Classification of Rocks.

The earths which produce the minerals that form the ordinary or essential ingredients of rocks, are silica, alumina, lime, and magnesia. If the others are occasionally found, it is rather in those minerals which cannot be considered essential to the constitution of rocks, but which are frequently imbedded in them. To these earths must be added iron in different states of oxydation, and, from some observations which I have made, in that of a carbonat also. Potash and Soda are, lastly, essential ingredients in some rocks; and it remains to be proved whether Lithion may not sometimes be present where one or other of these has been suspected. As the earths, as well as the alkalies, are now known to be oxydes, and as it is also known that silica, at least, acts the part of an acid in some mineral combinations, it is probable that we have much yet to learn respecting the origin and formation of many rocks: but whatever splendid probalities may open on us from this new source of knowledge, we are scarcely yet able to build any rational conjectures on it.

The simple minerals formed of these substances, and which constitute the essential ingredients of all rocks, are quartz, felspar, mica, hornblende, hypersthene, diallage, augit, serpentine, compact felspar, actinolite, chlorite, talc, and schorl. Some of these are, however, far more abundant than others; nor is it easy to define the limit between them, and those which may be considered accidental, or which are occasionally imbedded in rocks as their natural repositories. It is sufficient to quote as examples, garnet, which is sometimes abundant in micaceous schist, or sparingly dispersed, or altogether absent, without affecting its essential characters, and spodumene or corundum, which may thus exist in granite.

If we consider the great number of minerals in nature, thus generally distinguished into essential and unessential, or if even we limit our views to those which may be considered as most essential, it is interesting to observe how few are the rocks which are produced from them. If the varieties are most numerous in the primary or older series, they are still confined, and, within certain limits of variation, very constant. In the later rocks, they are still more limited,

When we reflect on the circumstances under which the primary rocks at least have been produced, they are confined to a much less number than would have been anticipated. As most of the minerals of ordinary occurrence are formed, for example, of the earths which exist in granite and gneiss, we might have expected to find garnet, corundum, or andalusite, in every mass of these; instead of being, as they are, limited to a few occasional specimens. Nor is it always very easy to account for those distinctions between gneiss, micaceous schist, quartz rock, or other substances, which occur in the same antient series; distinctions which, on the great scale, are really steady and definite, notwithstanding the occasional interferences of character that occur in particular instances. That these have been regulated, however, partly by mechanical and partly by chemical laws, is certain; and though we cannot perhaps explain every case, it will immediately be seen that we are in possession of general principles applicable to the solution of the question at large. Original differences in the materials are the foundations of the leading distinctions; and the remainder must be sought in chemical actions. Thus also, changes or alternations are the results of changes in the materials, combined sometimes with variations of the chemical forces or affinities. Hence it also is, that rocks preserve the same characters wherever they occur; a circumstance otherwise calculated to excite our surprise. In every other department of nature, her productions vary according to the climate and situation, but granite is the same in Egypt and in Greenland. It is with the laws of organization alone that climate interferes.

As the secondary, or later, strata have been chiefly formed from the waste of these antient and definite rocks, it is less surprising that they should preserve a general constancy of character throughout the globe, however individuals may vary in different places. Even these variations are still remarkable; as well from their steadiness, as from the extent through which that uniformity can sometimes be traced. The difference between compact limestone and chalk, is no less remarkable than the similarity which, in distant places, occurs between strata which we can scarcely conceive to have formed parts of one deposit. It is worthy of remark however, that, in the secondary strata, the most conspicuous variations occur in the limestones; and these, it is obvious, have been subject, in many instances, to chemical laws, as well as to the influence of organized bodies, from which the others have been comparatively exempted. That the secondary strata should contain sandstone and schists, is easily accounted for, by recollecting that these must be the result of the destruction of the older rocks; the more durable mineral remaining distinct, while the compound ones have been reduced to powder. That they alternate, is the consequence, partly of distinctions in the deposited materials, partly of the hydrostatic actions by which substances unequally gravitating in water are separated, and partly, of animal growth and reproduction.

On the Consolidation of Rocks.

As almost all the rocks have been formed out of our sight, the mode in which the earths, or simple minerals, have become consolidated into these forms, is to us a matter of inference from analogy; not of observation. If discussion could have determined this question, it would have been solved long since; as most of the schemes which have been called Theories of the earth, have been chiefly engaged in this pursuit, and as neither argument nor assumption has been spared in attempting to establish the exclusive views of many of these theorists. To record the terms under which the different partizans have thought fit to array themselves, would be to foster and perpetuate an opposition, often arising, more perhaps from the colours of the different banners than from the merits of the cause.

Fortunately, all rocks have not been formed in the depths of the earth, and fortunately also, it is in the power of art to produce some of these substances from indiscriminate mixtures of their elements. It is our business to try how far we can extend analogies from the visible to the invisible, from the present to the past. If this process will not carry us far, it is at least the only rational mode of investigation in our power.

Volcanoes are among the most active and impressive sources of those rocks which are now daily forming on the surface of the globe. By the agency of their fires, the earths are ejected in a state which, as far as we know, is merely that of mixture, and united in the fluidity of fusion. By repose during a process of slow cooling, various combinations take place in these fluid masses; and, according to circumstances which we are but imperfectly able to appreciate, there are formed numerous rocks, either apparently simple, or compounded of the different minerals that have been formed by the contending affinities of the materials. These processes are imitable by art; which, having first reduced the natural compounds furnished in basalt or other rocks, to a fluid and uniform glass in the laboratory fires, disposes them so as to cool during long repose in a gradual manner. Thus, by the slow cooling of the most compounded materials of the glass-house furnace, various imitations of rocks are formed; and thus, more precisely, the greenstones of the trap family are destroyed and again regenerated.

In examining, now, those rocks which have been formed out of our sight, we find one family which produces many counterparts to the volcanic rocks, namely, the family of trap. So absolute indeed is the identity between many members in each set, that no eye nor any analysis can distinguish them. To attempt to prove this by an enumeration of specimens in each, would be only to give a list of names that would carry no conviction. But no more convincing proof is wanted than this; that, to this moment, geologists continue to dispute about what belongs to the trap family and what is of volcanic origin; not only in countries remote from volcanoes, or no longer containing the marks of former activity, not only in the Vivarais and the Euganean hills, but at the very seats of living volcanoes.

If therefore out of a common mass of rock, or among many different ones evidently formed under the same circumstances, there are parts which bear all the marks of an origin similar to that of volcanic rocks, it is evident that the whole must be referred to the same source, with certain exceptions arising from collateral circumstances which will find a better place in treating of this family hereafter. Thus analogy, resemblance, and experiment, confirm that opinion respecting the trap rocks which would be inferred from the peculiarities of their chemical constitution; and thus also they confirm the conclusions elsewhere drawn from their peculiar disposition, and from the nature of their connexion with the various conterminous rocks among which they are found.

It is but a step from the trap rocks to granite; and if the identity of specimens is not always so perfect, or the resemblance so general and extensive between these and the volcanic rocks, the analogical reasoning is quite as unexceptionable. I have shown in another place, (Chap. x.) that many rocks, forming integrant portions of a granite mass, are undistinguishable from many of the traps, and that among these, there are many that resemble the productions of volcanoes. Here then is an identity, even between granites and volcanic rocks; and, here also, what is true respecting the origin of one part of the mass must be true respecting the whole. If that inference appears to be drawn closer than the circumstances seem to warrant, we may carry it through the intermediate stage of trap; and having thus proved the identity of this rock with the volcanic products on the one hand and with granite on the other, apply a common mathematical axiom to the conclusion.

If it be said that volcanoes do not produce perfect granite, it must still be recollected that they produce compounds of an analogous nature in every respect. Faujas indeed had said that lavas never contained quartz, but Breislak has produced numerous instances of this; while Dolomieu mentions quartz, felspar, and mica, as forming the white lavas of Ischia, and describes some of them as being "almost granitic." It was also shown that the trap rocks often assumed the characters of perfect granite; so that, by this intermediate step, the several products which are most distant are again associated. Even admitting that the volcanic rocks stood exclusively at one extremity of a scale of chemical compounds, and the granites at the other, the trap rocks, containing examples of both, form the common link by which they are united. This view of the chemical origin of granite is confirmed by the same set of appearances which confirm it in the case of the trap family, and which are fully described in other parts of this work.

It is not difficult to assign probable reasons for the differences in the chemical appearances of the rocks in these three distant productions. They have however already been sufficiently pointed out; and it was shown that they probably consisted, in a great measure, in differences of the time through which the fused materials had cooled: circumstances confirmed by a great number of collateral appearances already mentioned; though in many cases, there can be no doubt that great differences have resulted from the different proportions of the several earths in the fused compounds.

It is unnecessary to repeat, that the production of granite from solution in water, is incompatible, equally, with its mineral and geological characters and with the laws of chemistry; because negative arguments can have no weight with those who form, what are popularly called opinions, without evidence, or against it. To the insolubility of the earths, and to the impossibility of thus producing a simultaneous and confused crystallization, it is unnecessary to add that of the abstraction of the solvent. Those who have retorted on the theory of consolidation from igneous fluidity, that quartz is not fusible, have only shown that ordinary ignorance of chemistry which has attended most of these disputes; by not knowing that the earths separate from the general mass, to form minerals, according to the laws of chemical affinity.

Thus, from chemical analogies, there is assigned to all the unstratified rocks, that origin which was already deduced from various other considerations: and thus there is proved to exist a division of rocks formed exclusively by the agency of heat. It will now be convenient to begin the remainder of this examination at the other extreme.

Where water holding carbonat of lime in solution is gradually evaporated, there are formed calcareous concretions which often attain a great size through age, and which, under peculiar circumstances of crystallization, are sometimes not very different in aspect from certain limestone rocks. Under different circumstances, similar waters deposit their contents, so as to form rocks of great depth and extent, producing real calcareous strata. The Travertino of Italy appears to be one of the most perfect examples of this nature. These simple and recent calcareous rocks become compounds, in cases where the calcareous solution has entangled fragments of shells, as it does in the West Indian islands at this day, or where it has united fragments of discordant natures, as it does on the shores of Messina and on many of our own sea coasts. Thus calcareous rocks, both simple and compound, are formed by water. Lastly, rocks of this nature are now daily produced in many parts of the great ocean, by the efforts of marine animals; the deserted coralline structure being cemented, partly by the actions of the animals themselves, and partly by that of the sea on the calcareous earth. In the same manner, antient submarine piers, as at Carthage, become cemented through lapse of time, into masses of solid rock, by the intervention of shell fish and the solution of their calcareous matter. In this way, calcareous rocks are formed, partly by chemical agency, and partly by that of submarine animals.

Where iron becomes converted from the metallic or oxydulous state to that of rust, it becomes the cement of all the smaller materials within its reach; and thus sandstone is often formed on sea shores, in sand and gravel beds, and, very probably, to a considerable extent, in the noted ferruginous sand stratum of England.

Thus two modes of producing rocks by the agency of water are demonstrated. It remains to inquire what probability there is that the same agent can convert silica to that end; as we cannot produce any instances so perfect of its absolute action in that way.

The solubility of silica in water cannot be a matter of dispute, however difficult it may be to effect its solution in our laboratories. In the chapter on the formation of veins, I have produced nearly all the instances of this nature that are required for the present purpose; but I may here add to these, its actual solution in the hot waters of Iceland and Italy, and the consequent production of siliceous tufas and stalactites. To convert this property to the present purpose, it is not requisite that the solution be very extensive, or very rapid. If we conceive this agent operating for a long series of years in a mass of loose sand or of clay, it is not difficult to see that the final result must be, in the first instance, the formation of a sandstone, and, in the other, probably, that of a schist. That this is the fact in nature, is almost demonstrable from the frequent partial occurrence of sandstones in beds of loose sand, and from the mixed chemical and mechanical texture of almost all the solid sandstones in nature. This effect, it is true, has been attributed by certain philosophers to the action of heat. But to adduce as an agent, that which cannot be shown capable of producing a given effect, while we are in possession of one that has the desired power, is to abandon sound reasoning for the sake of maintaining a species of fictitious analogy, which, after all, is not necessary for the support of that theory by which it was so anxiously defended.

Thus there have been demonstrated two distinct sets of causes for the formation of rocks; the first chiefly applicable to the unstratified substances, and the last to the formation or consolidation of strata.

It has been objected to the possibility of aqueous consolidation, by those who have laboured much more to dictate a system of geology than to deduce one, that a liquid solvent could not exclude itself from the pores of the rock after depositing the consolidating matter; that it should therefore remain within the stone, or else leave the body pervious to water; "neither of which is" said to be "the fact." On the contrary, both of these are facts. That which was asserted not to exist, by those who did not know what did exist, and reasoned as indifferently when they knew it, is true. The presence of water in stones is so universal that I have never yet seen any rock in which it was not found, when that could be procured quickly from a sufficient depth. It is contained even in granite and in the trap rocks; and the great change of colour and hardness which many of the latter undergo after being formed into specimens, is owing to its evaporation. Thus, specimens of augit rock, which have the waxy, soft look and green colour of serpentine, when fresh broken, become black in a few days. It was mentioned when treating of the flexibility of rocks, that small granite veins were sometimes found perfectly soft in the quarry; and these harden in a few days, apparently by the evaporation of their water and the consequent precipitation of silica, or else by the nearer approximation of their parts. In Sky, as I formerly mentioned, I have found masses of granular quartz, or sandstone, which could be moulded by the hand when first taken from the earth, but which, in the same manner, became solid in a few days. In all these cases, the loss of weight proves the presence of water, as it does the porosity of the stones. Even the common quartz of veins contains water under the same circumstances; losing both weight and transparency on drying, as I have proved. The porosity of stones, as well as the presence of water, are, thus, both demonstrated by the same facts. But the former property ought never to have admitted a doubt; since the compactness of flint and agate are apparently far greater than that of any rock, compound or simple, and since these, give passage to water, to oil, and to sulphuric acid, as I have shown in an essay on this subject.

That the water in stones is actually saturated with earths, and probably with silica or lime, appears to be also proved by certain appearances which take place on breaking and drying some of these. In marbles raised very wet from the quarry, a whitish dusty surface soon follows, from the deposition of the carbonat of lime; and a similar deposition of silica will account for that grey tarnish which is produced on pitchstones within a very few hours after the specimens are broken from the rock; during which process of drying they also become far more compact, or less tender. Thus the objection in question falls to the ground; were it even necessary that the process of consolidation should be reserved for that time at which the whole stratum was completed. It is quite easy, on the contrary, to conceive that it has, in many cases, proceeded gradually, even during the deposition, as it actually does in the case of travertino. It is a very lax chemical view of such a process, to imagine the necessity of a solvent entering to deposit its contents, and then "excluding" itself.

Of the different Rocks, and the Modes of their Consolidation.

Now, though a large portion of the strata of the globe may have been brought into that form by this last process, or by aqueous solution, and that a considerable portion, at least, of the secondary ones probably owe their origin or consolidation to this cause, there are many strata, particularly in the primary or older series, to which it is impossible to apply it so as to explain all the appearances which they present.

There is nothing in the character of quartz rock, as far as I have examined it, to prevent it from having been consolidated to its present condition from the long-continued application of an aqueous solution of silica. But that it was deposited from water, originally, in the state of sand and gravel, is rendered evident from the rounded and foreign fragments of discordant rocks which it often contains. At the same time, there is no reason to deny that it may have been exposed to the action of heat, as it is still capable of undergoing that without suffering any change. That it was consolidated by heat we cannot prove; and are scarcely in a condition to deny that it may have been partly indebted for its constitution to that cause. We are indeed almost certain that quartz sand can be converted into solid and continuous quartz by heat; since, as elsewhere noticed, analogous effects take place in the vicinity of trap, and as, in Fife, a bed of such quartz is found among the coal strata, in the neighbourhood of that substance, probably indurated from the state of an ordinary sandstone.

If shale could be indurated from water alone, there would be no reason to deny that the same cause may have operated in the primary argillaceous schists; while, that they have been deposited from water, is proved by the fragments and the shells which they so often contain. Here again, however, we are in the same condition as with regard to quartz rock; unable to prove that it may not have experienced, in some degree, the action of heat, as we know, from observations on the siliceous schists, that shells are not necessarily obliterated in these circumstances. But that action, if it existed, cannot have been very great; as we are certain, both from experiment and observation, that it is either fused or indurated to siliceous schist by this cause. The very existence of that substance in the vicinity of trap and granite, produced by the action of these rocks on shale and slate, not only prove this fact, but show the very limit where the action of heat ceases. Yet I must remark, that the action of heat on the argillaceous schists may have been short of producing this effect, and yet have caused others analogous to those which occur in micaceous schist. Such is the formation, within them, of hollow spar or other minerals, which we cannot attribute to any cause but this.

Thus, two important members of the primary strata may have been indurated from water alone; and yet they appear also to have, in some cases, been affected by heat, perhaps even indurated by it. With respect to limestone, it is now known, both by direct experiment and by observation on the effect produced by trap veins on chalk, that it may be crystallized from fusion, provided the escape of the carbonic acid is restrained. It has been shown that it is equally consolidated from water; and on examining this limestone in its various associations, its origin must probably, in some instances, be referred to one of these causes, in others, to the other. It is probable, for example, that all the limestones associated with clay slate are derived from watery deposition and crystallization; though even these may, like the slate itself, have been affected by heat; but it is much more than probable that those associated with gneiss have received their present condition from the latter agent. This opinion is justified by many circumstances; such as by their giving passage to granite veins, by the change of chemical texture and composition which they present in these cases, and by the crystallization, within them, of minerals similar to those found in gneiss, such as garnet, hornblende, augit, and others, which could not have been deposited from water so as to have entered into the confused crystalline arrangement of the rock in the manner which they do. That limestone is actually thus consolidated after fusion, even in large masses, is also proved, as far as any thing relating to the influence of trap is proved, by the conversion of conchiferous secondary strata, in those situations, into crystalline limestone; a fact occurring very extensively and demonstrably in Sky, and recorded in my work on the Western Islands.

With respect to Serpentine, the whole question is as yet involved in darkness. It is not known that it can be formed from water, and I have proved, that as it passes into trap, it can be formed by fusion.

All the scaly schists, of which micaceous schist may here represent the whole, present characters which are scarcely explicable without admitting the action of both these agents. The stratified disposition and the laminar form, give indications of a deposition from water; and if any doubt of that could remain, it is removed by finding that, in many places, it contains fragments of discordant rocks, of granite, for example, limestone, and quartz rock. It has further been held, that the parallel position of the mica is in itself a sufficient proof of deposition, because, it is the necessary position, and because the same circumstance exists in the micaceous sandstones, so analogous to it, which are actually deposited from water. But this, if probable, is an equivocal circumstance; as I have observed that, in hypersthene rock, a member of the trap family, and even in some rare trap veins that contain mica, in the Western islands, as well as in veins of antient porphyry, the flat crystals of hypersthene in one case, and the mica or pinite in the others, preserve that parallelism which must here be attributed to the polarity of crystallization operating extensively; an action which I have also elsewhere shown to have been sometimes exerted throughout the felspar of granite veins.

But admitting now, as it cannot be denied, that micaceous schist was deposited, like the secondary micaceous sandstones, from water, and consolidated by the same means, it presents characters which cannot be accounted for by this process. If its flexibility has not been the consequence of heat, which I have elsewhere attempted to prove that it has, the peculiarities of its crystalline texture and occasional contents cannot be explained, without admitting that it has been exposed to a heat, sufficiently intense and sufficiently durable, to permit these minerals to be formed in the same manner as they are in granite and in the volcanic rocks. The condition and existence of garnet, hornblende, tourmaline, staurotide, and other minerals, are inexplicable by any mode of watery deposition, and still less by any subsequent crystallization from water.

This argument, which applies generally to all the stratified rocks that contain superfluous minerals, crystallized within them, and not rounded by transportation, is illustrated by what happens in cases where strata have been exposed to the action of trap rocks, and where the influence of heat is more generally admitted. Thus, at Poussac, near Bagnières, there are limestones which have been affected, and indurated, or otherwise changed, by the contact of basalt; and it is here remarkable, that while the purer ones are simply rendered crystalline, the earthy produce macle, actinolite, and tremolite. This is exactly analogous to what happens where granite invades primary and earthy limestones; and where the partial change, and similarly partial presence of such minerals, confirm, both this view, and what was just said respecting the primary calcareous strata themselves.

As to diallage rock, and the more antient red sandstone, the same processes of reasoning apply to them as to those rocks to which they are analogous; but hornblende schist requires a particular consideration. This is an extremely fusible compound, and its peculiar crystalline texture proves that it could not have been deposited from water; in which indeed its earths are insoluble, and from which they could not have been precipitated in such a form. It is, besides, precisely analogous to many greenstones of the trap family; from which indeed it is often so little distinguishable, that it has been confounded with them under the name of primitive greenstone. That it is, further, actually produced by heat, is evinced by finding that the argillaceous schists, when in contact with granite, are actually converted into it. Whether simple, or compounded of hornblende and felspar, the same reasoning applies to it. It is nevertheless admitted, that its original materials have been deposited from water, and thus its laminar and stratified disposition is explained. That it has, further, consisted of clay or schist, is rendered probable, not only by the numerous facts occurring in the trap rocks, but by that very striking analogy in which beds of shale beneath trap are actually converted into Lydian stone; a substance differing from it, almost solely in the compactness and uniformity of its texture.

But indeed, as far as a single fact can prove such a case, the origin of hornblende schist from clay slate is completely established by the occurrence, in Shetland, of a mass of the latter substance alternating with gneiss and approximating to granite. Here, those portions which come into contact with the latter, become, first, siliceous schists, and, ultimately, hornblende schist; so that the very same bed which is an interlamination of gneiss and clay slate in one part, is, in another, the usual alternation of gneiss and hornblende schist.

We thus lastly arrive at gneiss; a rock which often bears the marks of igneous consolidation in a still greater degree than those of aqueous deposition, but in which it is almost unquestionable that both have been combined. Where gneiss is at a distance from granite, its laminar and stratified disposition is most perfect; where in its vicinity, that is most obscure; so obscure indeed as at length to disappear. This is precisely what might be expected to happen on this view of its double origin; namely, the application of heat in unequal degrees, to a series of beds deposited from water, and probably, like quartz rock, originally consolidated from it also. Where it is most remote from granite, although its mineral materials should be the same, they are disposed in a different manner; or are more rigidly laminar and more independent. Where it is most immediately in the vicinity of that rock, and more particularly when it abounds in granite veins, the structure becomes analogous to that of granite, or to one in which there is that mutual penetration of crystals which can take place only in a fluid of fusion. At length it actually passes into the contiguous granite; losing that parallelism of the parts, and those last remains of the laminar disposition, which had gradually been decreasing.

It is by no means difficult to imagine this combination of causes and of effects: a state of softening or semifusion, sufficient to allow the integrant parts of a stratified watery deposit to enter into new combinations, and to recrystallize without the loss of the original marks of stratification. These indeed are often preserved in gneiss, by the alternate interposition of beds and laminæ of hornblende, and by that only; just as, in the watery joint deposit of sandstone and shale, the latter substance is often the only indication of stratification that can be procured. That such recrystallization can take place in a rock which is heated to a point short of actual fluidity, is proved; and that strata can, in nature, lose all their indications of watery deposition, while they preserve the stratified shape under a new mineral form, is evinced by the existence of siliceous schists beneath trap, already quoted. A greater degree of heat and a longer continuance of it, are all that are required to produce the differences in these cases; and the fact of the frequent interposition of hornblende schist between beds of gneiss, is strongly confirmatory of the consistency and truth of these views. Thus also the transition of gneiss into granite becomes a phenomenon of easy solution.

Of the general Causes of Consolidation.

I need not here terminate this view of the consolidation of these primary rocks, by any general inquiries respecting the origin of the heat or its diffusion. Nothing can be said on this subject that has not been said elsewhere; and whatever difficulties may occur in attempting to apply these principles rigidly to every case that may be examined, it can only be said, that this theory offers a general and obvious solution of the facts; and that if it cannot be exactly fitted to meet every exigency, it is no more than must happen in every similar case of a general principle, when we are not in possession of all the collateral circumstances by which it may have been modified.

In thus deducing from the agencies, both of heat and of watery solution, the consolidation of all the stratified rocks, and in limiting these according to the various circumstances which have been indicated, it must be apparent that the power granted to the former is comparatively small, and that it is not here supposed to have acted beyond the range of the more antient rocks, probably not through the whole of these.

That it has operated in the consolidation of the secondary strata at large, is rendered improbable, by a variety of circumstances which I need not enumerate, because they have frequently been urged against the whole theory.

But in admitting that the great mass of the secondary strata has been consolidated by a watery agent it must be remembered that there is a wide difference between the consolidation, and the precipitation of the same substances from water. If every one of these rocks did not give the most unquestionable proofs of its having originated, either in the ruins of more antient rocks or in the spoils of animals, it would be a sufficient argument against precipitation from a watery solution, that it involves every species of chemical and mechanical impossibility that can be included in a proposition so simple. It is unnecessary at present to detain the reader a moment longer on an hypothesis that would create and destroy oceans at its pleasure, yet find them ineffectual.

It has indeed been suggested, that if the original heat of the globe was considerable, the water must have formed an atmosphere producing a degree of pressure that might have rendered many earths far more soluble than we know them to be, in what remained fluid of that water, thus heated to some unknown temperature. On a speculative opinion of this nature, I need offer no remarks, in an inquiry which rests on better kinds of evidence and probability.

No notice has yet been taken of the power of mere pressure, either in actually consolidating rocks or in assisting their consolidation. Yet it is an agent not to be overloooked; and when we consider the enormous weight to which the strata must have been subjected, it is very easy to conceive that its power cannot always have been inefficient. The occasional compression and fracture of imbedded shells, proves that it has sometimes really acted; and if even the most delicate of these bodies are generally preserved, it only proves that they were well supported by the surrounding materials, not that they have not been subjected to great pressure. In our own experiments, with forces far inferior, clay can be compressed into a substance as hard as shale; and there are many of the schists not so hard as the heterogeneous mixture which is forced into a rocket, though composed of materials from which such an effect could scarcely be anticipated.

Of the Analogies among different Rocks, and of their Resemblances to unconsolidated Strata.

Having thus attempted to assign probable causes for the consolidation of the original materials of rocks, it will not be useless to attempt to trace these through their progress; to inquire if it is possible to discover, in the component parts and disposition of the most antient rocks, any resemblance to the loose matters which are now daily deposited beneath the waters of the present earth.

If we examine the deserted seat of an inland lake, we discover beds of compact mud intermixed with leaves, or of mud with land shells, or of sand, or of peat, or of all these, in one or more series of alternations. Abstracting the question as it relates to peat, we have here an analogy to the rocks of a coal series. The mud with its plants, or shells, represents the different shales and limestones; and the sandstone is the counterpart of the sand bed. The whole requires consolidation only, to render it an ordinary series of rocks.

In sinking through the ancient æstuaries of the sea, long filled up and converted into dry land, similar beds of mud and clay, of marine shells entangled in mud, and of sand and gravel, are found; varying in number, in thickness, in the order of repetition, and in the quality or nature of the remains, in almost every place. It is unnecessary to point out more distinctly a similar analogy in these preparations for a series of secondary rocks; but it obviously requires only a repetition of the same deposits, sufficiently frequent, to produce the whole series of secondary strata. At what period the act of consolidation may have taken place, we have no means of knowing; yet as far as our observations have yet reached, we have no positive evidence that any extensive operations of this nature are now going on, excepting those formerly mentioned. The process may possibly be too slow to fall within the sphere of our investigations.

If the more antient strata have been formed from similar materials, they should possess an analogy to the secondary; and, admitting such differences as may be accounted for by the circumstances of difference in respect to consolidation to which they have been exposed, we should find among them a set of alternations analogous to the sandstone, shale, and limestone of the latest series. Such an analogy can indeed be traced, nor is it very imperfect. It must be obvious, that a great part of the differences that do exist, is explained by admitting the effects of heat on them; and it may be supposed that there have also been some differences in the rocks from which the materials of these strata were originally deposited. But, as to the whole mass, there is still one leading difference remaining, of great importance, and on which some light may at least be thrown, if it cannot be fully explained. It consists in the very great disproportion of limestone in the two series; that rock being abundant in the later, and comparatively rare in the earlier strata.

The formation of Coral islands proves that enormous and solid masses of calcareous rock are the produce of animals alone; and when we reflect on the magnitude of some of these, we have no reason to be surprised at the extent of those rocks which, among the secondary strata, are composed chiefly of shells. Were we even to suppose that every particle of the largest bed of limestone known, was originally part of the body of a shell, we should, as far as the bulk of the mass is concerned, assume nothing that would not be countenanced by the magnitude of the great coral reef of New Holland. If the most minute animals of creation can thus, by their numbers, execute, unassisted, works of such enormous magnitude, and, as navigators think, within spaces of time comparatively limited, it is far from unreasonable to believe that the succession, through unnumbered ages, of animals so far exceeding these in bulk and in the relative quantity of their calcareous produce, should have generated all the calcareous strata in the secondary series.

It is not necessary here to ask whence the calcareous matter has been derived, or to suppose that it is an animal product. That difficulty is at present, unquestionably, insurmountable; but, in this case, it is of no moment. It can form no objection to the power of oysters or pectines in producing, by their own energies, a bed of limestone; because the fact, however inexplicable, is rendered unquestionable by the generation of coral from sea water. That very extensive beds of calcareous matter may be produced by animals, and from their remains, is also incontestably proved by the oolithe limestones, and by those deposits of shell marl so often found in fresh water lakes. In many such cases in the Highlands of Scotland, it can easily be demonstrated that this is their sole origin; because we can trace the courses of the streams by which the lakes have been fed, and ascertain that they could not have carried down calcareous matter; their origin and progress lying among siliceous strata.

It must be admitted indeed, that whatever calcareous beds may be at this moment preparing at the bottom of the ocean, the probable germs of future strata, they will be formed, like the shales and sandstones, from the ruins of the present calcareous secondary rocks; and that the operations of shell fish will constitute only a part of the causes of their production. Nor need it be denied that such has been the case to a certain degree in former times: but that the assistance afforded by the ruins of primary calcareous rocks has been very trifling, will appear evident from a mere arithmetical comparison which can scarcely deceive.

Every thing proves that the present secondary strata are the produce of more antient rocks; and these must have been the immediate successors of those which are now the primary; as we have no reason to imagine that there has been a distinct series which has entirely vanished. The proportions of the different materials in the produce, ought therefore to bear a certain relation to those in the original repositories; or, if there was a difference, it would be expected to be in favour of the most yielding materials, schist and limestone. But if we examine the quantity of limestone in the primary strata, it will be found very small. What the exact proportion to the other rocks may be, throughout the world, is not known; but, in Britain, it certainly does not amount to a ten thousandth part of the whole. But among the secondary strata of England, the limestones bear a far larger proportion to the siliceous and argillaceous rocks. If we were to assume only the ratio of one hundredth, it would answer the purposes of the present argument; and there is nothing unreasonable in referring the origin of the British secondary strata to the British primary rocks, with the necessary exceptions, of course, as to these calcareous strata. This however is a matter of indifference; as the general fact, taking the whole world, is indisputable.

Thus it may fairly be inferred, that while the siliceous and argillaceous secondary strata have been formed from the ruins of more antient rocks, a large part at least of the calcareous, is the produce of animals. Hence also it must appear, that from the operations of animals, the quantity of calcareous earth deposited in the form of mud or stone, is always increasing; and that as the secondary series far exceeds the primary in this respect, so a third series, should one hereafter arise from the depths of the sea, will exceed the last in the proportion of its calcareous strata. It will combine the ruins of the last limestones with the spoils of the present animals: animals, of which the generations are also probably enlarging and extending in every age, in a ratio proportioned to the increase of those calcareous, or soft alluvial and submarine deposits which they affect and favour. Those who extend the prophetic eye of philosophy to worlds yet unborn, may also thus anticipate a constant and steady approach to that universal state of fertility which is now the character and the pride of our calcareous soils.

If we now turn our views backwards to the primary rocks, we find, in the disposition of their limestones, a confirmation of this opinion respecting the important agency of living animals in the production of calcareous strata. It has always been believed, or at least asserted, by geologists, that no animal remains existed among the primary rocks; and to avoid a breach in this hypothesis, among other reasons, the *transition* class was invented. I shall not here discuss the truth or the utility of this invention. It is sufficient to say, that the schists containing shells appertain to those rocks admitted to be not secondary, and that the only general revolution among the strata which we know, is of a later date than these. So far as the present purpose is concerned therefore, the animal remains of the schists are primary, in as far as they are prior to the secondary strata. Nevertheless, the animal remains of the primary strata, admitting among them those now named, so as to give the most favourable colour to the subject, bear a disproportion to the whole of the rocks, not unlike that which the limestones do to the siliceous and argillaceous strata. This should be expected from the presumed rarity of these animals in the antient ocean.

It has been supposed by some geologists, that all the calcareous strata, of whatever age, were the exclusive produce of animals. That possibility is countenanced by the phenomena of the Coral islands; though the accessary causes, arising from the decomposition of previous limestones, must be admitted as far as regards the secondary strata. But the mere existence of primary limestones thus operating by their destruction to assist in producing new ones, is not itself a proof that these are original, and independent of animal sources. The existence of animal remains in primary schists has just been mentioned; and I have elsewhere described one instance in which these occur in a calcareous quartz rock situated beneath gneiss. Thus far, they might have contributed to the production of even the primary limestones; and if they are not more frequently found among them, causes for that are not wanting.

In the first place, primary limestones are, not only comparatively rare, but geologists having adopted the hypothetical opinion that they ought not to contain animal remains, make it a rule invariably to rank such instances among their *transition* series; without thinking it necessary to investigate the subject by the rigid rules of pure geological analysis: by position, and relation towards the neighbouring strata. It is further obvious, that the primary rocks have undergone great disturbance, and, in many instances, serious changes; and, even among the secondary strata, in such cases, the animal remains are often obliterated. The fusibility of limestone has already been demonstrated; and it has already been proved, that many of the primary strata bear marks, scarcely to be disputed, of the action of long-continued heat. Thus it is to be expected that their organic remains, if they ever existed, should have been obliterated; and if this has not happened in the case just quoted of shells under gneiss, it may be attributed to the nature of the bed in which they lie. Proofs of the truth of this view are found most distinctly detailed by nature in Sky, and in the Isle of Mann; as I long ago indicated, and as has been since confirmed on the continent of Europe. Where the conchiferous beds are actually converted into pure crystalline limestone by the action of the incumbent trap, that is undistinguishable from the primary rocks of the same kind, and all the shells have disappeared; while, in some parts of the gradation between the stratified and fused rock, their gradual loss of form, and final obliteration may be traced.

Having thus disposed of one great branch of the analogy between the primary and secondary rocks, it is necessary to see what may be inferred respecting the remainder.

The difference between shale and slate, or between the primary and secondary argillaceous schists, is often so small as to have been a source of error, even among experienced geologists. If, when separated from their connexions, there are specimens, particularly among the oldest of the shales, which no care nor practice could distinguish from the primary schists, the resemblance between the sandstones and quartz rock is often equally accurate; though, in a general sense, the latter is distinguished by its superior compactness and more predominant crystalline texture. Where quartz rock contains mica, it may be compared to the micaceous sandstones, from which it, in fact, differs only in compactness; and, where felspar is an ingredient, it is obvious that it bears an analogy to the argillaceous ones.

Here then, in primary limestone, quartz rock, and argillaceous schist, we trace an analogy, not of a very remote nature, to the secondary strata; showing that, with certain variations, from causes not difficult to comprehend, nature has repeated herself at considerable intervals of time, and has been guided by laws of great general simplicity. It remains to extend this analogy one step further; but the difficulties increase, as might be expected, at each remove.

In micaceous schist, we find an analogy to micaceous sandstone too obvious to be disputed; and whatever varieties of composition it may present, they depend on different proportions of the micaceous ingredient; the predominance of which, in particular cases, may probably be attributed to the nature of the rocks from which its materials were derivéd, possibly from the state of heat to which, as before remarked, it has been exposed. Its other peculiarities have already been explained in a similar way. Gneiss, if we consider its materials, holds a parallel with a sandstone containing clay and mica; and here, although the analogy becomes finally very feeble, there is a chain through the varieties of this rock which connects it with the secondary sandstones as perfectly as quartz rock is thus traced. The causes for the evanescence of this analogy, have been already shown to consist in the posterior influence of heat, causing it to approximate to granite, and, finally, to graduate into it. It was then also shown, that the action of heat converted shale into hornblende; and thus, in the frequent alternations of gneiss and hornblende schist, we have an exact counterpart of that alternation so common between the oldest of the secondary sandstones and its concomitant shale. I need not dwell longer on this question as far as regards detail; but the whole admits of one general conclusion, considerably at variance with the popular opinions of geologists and systematic writers respecting the differences of rocks.

It has been always said that these differences were inexplicable, and that we could not account for the great variety of rocks in nature, still less for that constancy which they retain under their variations, nor for the alternations of the different kinds. Now I have shown, that as far as their varieties are concerned, we have, with magnesia, but three materials, lime, clay, and quartz, and that these, either simply, or in combinations which are chiefly binary and between the two last, form the leading materials of all the rocks. In the purely mechanical, or in the aqueous rocks, the variations could be little more than variations of proportion, in those formed of more than one material; and since the general law of deposition and consolidation has been nearly uniform, a corresponding uniformity of result is a necessary consequence. In the more properly chemical rocks, or in those which have participated in the effects of heat, a simple law also, varying merely in intensity, and always acting on the same limited materials, differing merely in their relative proportions, has almost necessarily produced corresponding or analogous results. Taking granite thus as the extreme case, the effect of this agent has been to recompound the two leading materials which it found, namely silex, or quartz, and clay, into felspar, mica, and hornblende, of which two, at least, form also the essential parts of the other extreme case, trap; the cause of difference here also consisting in other well-known materials. In the intermediate cases, it has produced gneiss and hornblende schist, or micaceous schist; differences similarly resulting from a nearly common action on materials differently apportioned. And further, as far as the aqueous or chemical rocks contain the minerals of granite, or the original materials not in the simple forms of clay and sand, they are indebted for them to that very igneous source, however circuitously and remotely. Thus the laws and the materials have been, throughout, simple, and, consequently, the constancy of these variations could not have been otherwise. I have already shown that the rocks themselves are all connected in leading analogies, or resemblances, throughout the whole series, and are, virtually, reducible to a few, instead of presenting that great variety which is popularly imagined. The merely simple rocks require no remarks; as their resemblance through the whole series, is such that we often cannot distinguish them, except by position.

Now, with respect to the difficulties supposed to consist in their alternation, I have equally shown that this has been regulated by one leading law, and that what remains may be accounted for by the operation of another, equally simple. Hence the results ought to be simple; and this they actually are, when justly viewed. The separation of clay and sand, in consequence of their different powers of gravitating in water, is the leading law; and its modified effects are the binary, but differently proportioned mixtures which exist. This also applies to the mixtures of antecedent minerals found in the strata. The case of limestone from organic bodies is too obvious to require repetition. The second law, is the influence of heat in changing the characters of different simple deposits, or of differently compounded ones, whether these be rocks actually fused, or strata merely influenced; and this also being uniform, under gradations, the results should correspond.

Such substances as diallage rock, and serpentine, or rocks which are essentially characterized by such minerals as hypersthene and augit, are the real exceptions to this simplicity, and they are but a few in the total system; while, with perhaps no exception, being the produce of perfect fusion, it is less difficult to comprehend why they should have formed this breach in the general simplicity which I have thus attempted to establish.

Although it has here been inculcated that all the stratified rocks which are not the produce of animals, have ultimately been derived from former rocks, and probably, in a series of succession, the limits of which we cannot pretend to conjecture, it is still proper to remark, that there is a progressive change of character as we retreat. The limestones, it has been particularly shown, become more rare, but the argillaceous substances diminish also; so that, at length, in arriving at that antiquity which, to our observation, is the highest, siliceous rocks predominate in a great degree. Thus a certain philosophy might extend the conclusion formerly suggested with respect to the increase of calcareous strata, and imagine an universe once as incapable of maintaining vegetables, as it has, to all appearance, been limited in the numbers and nature of its animals; a desert of rocks and sand. But this conclusion is not justified when we take a general view of all the phenomena which geology presents. That it has been drawn, has arisen, either from false theories or partial views. If the siliceous substances predominate in the more antient parts of the series, it must be remembered that these are but the remains of rocks, of which the greater part has disappeared to form the present secondary strata; nor, in the revolutions of ages, can we decide on what has vanished and what the state of the more antient surface was. That it furnished a vegetable creation, and to a great extent, is evinced by the phenomena of coal strata, and by the enormous masses of vegetable matter deposited through uncounted ages, and amid a series of partial revolutions of which we can scarcely form an idea.

Of the Formation of conglomerate Rocks.

Though it has thus been shown, that with certain rocks more or less completely furnished by animals, the secondary strata consist of the ruins of more antient ones, it is necessary here to bestow a few paragraphs on the conglomerate rocks, since they present some peculiarities of origin that require notice, and since they offer the most perfect evidence of the mechanical nature of the process by which the strata have been principally formed. It is indeed by tracing the gradation from the coarsest conglomerate, formed of many discordant rocks, to the finer sandstones, that we become convinced of the truth of this supposition.

As also it was attempted to trace in nature the analogy between the finer rocky strata and the present deposits of sand and clay from water, so, in the superficial or deep-seated alluvia of a coarser nature, we find the prototypes of the present conglomerates; of the consolidated alluvia, whatever their origin and position may have been, of former worlds. The nature of the evidence which these rocks afford with respect to the revolutions of the earth's surface, will be considered in a subsequent chapter, (Chap. xxi;)

but it is necessary here to distinguish between those which are of a local and those which are of a more general nature.

These rocks are found, both in the antient and recent series; and, in both, under circumstances precisely similar, if differing in extent. They are properly divisible into general and local; and it is only indeed by thus distinguishing them, that we can derive any advantage, in our reasonings on events, from the evidences they afford, or avoid the confusion to which, from incorrect observation, they have frequently given rise. As, in both the secondary and primary series, similar accidents have occurred, in the fracture, displacement, and transference of strata, it is natural to expect that the conglomerates, here called local, which have resulted from these changes, should be found in both. With respect to the general ones, as they have been produced by that gradual waste of the solid rocks which now forms our superficial alluvia, it is natural to expect that they should be found chiefly, and most extensively, at the great interval which separates the primary and secondary strata; and this expectation is realized by the existence of that almost universal conglomerate, the first portion of that red sandstone, which is, itself, the lowest and first of the secondary series.

If no revolution of so general a nature can elsewhere be traced, yet partial ones of an analogous kind are found, both in the primary and secondary series; and thus, in both, there exist conglomerates which, if not universal, are still, in the sense here laid down, entitled to the name of general.

The mechanical origin of all these rocks is so obvious, that it is unnecessary to dwell on it; while it is also easy to discover that the component parts have undergone greater or less degrees of attrition; and, in many cases, of transportation. It has also been shown in another place, that, with the exception of the Tuff of the overlying family, they consist, in most instances, of different ingredients; and, not unfrequently, of a great number intermixed.

Those which consist of many different fragments, or even of fragments of two substances, may be considered as general conglomerates. They are, in a geological sense, only modifications of the different recomposed rocks with which they are found associated; and thus, like these, they necessarily occupy extensive spaces in nature. They may thus be distinguished from the local conglomerates, by their geological positions and connexions; while they may also, in a great measure, be recognised by their mineral structure; chiefly, indeed, by the attrition, whether greater or less, which the parts have undergone, and by the variety of ingredients which they contain. These remarks apply principally to those conglomerates which are found among the secondary strata, where different kinds or series meet, and, above all, to the old, or lowest, red sandstone, of which they often form very conspicuous portions. Those which are connected with the Overlying rocks, like the Tuffs of the same division, are distinguished by such peculiarities of character as to admit of no comparison with any others.

The local conglomerates, on the other hand, may be distinguished by their much greater variety as a class, and by the much more limited variety of their ingredients, sometimes consisting of only one, occasionally of two, but rarely exceeding three. The general conglomerates are also commonly composed of materials agglutinated without an intervening cement; whereas most of the local rocks of this character consist of one or more sorts of fragments united by a third cementing substance, or by a cement composed of one of the imbedded ingredients. The local conglomerates rarely occupy any considerable space, and are often very limited; while they are always attached to some simple or compound rock with which, in some parts, they are intimately united.

As the general conglomerates constitute a separate and independent set of strata, the local rarely form more than one bed, and are sometimes not even found in the shape of a bed; constituting a single lamina only, adhering to a parent rock, or an irregular mass, in some other way connnected with it. The general ones frequently contain rounded masses, but the fragments of the local are commonly angular, or little affected by attrition. In many instances they are perfectly acute; while, occasionally also, when of large size, they are found to be so little moved from their places, or separated from each other, that the imagination easily replaces the detached parts.

These rocks have been sometimes distinguished by the name of Breccia, while the others have been called pudding stone; but as the term Breccia has also been very indiscriminately used, it is not convenient to perpetuate its application where it is necessary to be accurate. Circumstances occasionally visible in the secondary strata, and more particularly in the calcareous, will explain the origin of the local conglomerates.

The beds of these are often found covered on the surface by their own fragments, intermixed with minuter particles of the same, or of clay. The imaginary consolidation of such a mass, would form a local conglomerate; and thus it may be understood why the angles of the fragments are so little rounded, and why the separated parts are so capable of being re-adapted. It is easy to conceive also, that the infiltration of a solution of lime would convert them into a solid rock, and that the same effect might, under other circumstances, take place from carbonat or rust of iron, or from some other of the causes which produce the consolidation of rocks. The several conditions thus hypothetically stated, appear to have frequently existed in nature; and thus have arisen the number of local conglomerates now seen.

The fractures of the rock, and the consequent production of fragments on the surface, have probably, in all such cases, originated, jointly, from the ordinary causes of waste, and from mechanical violence. In some instances, where the conglomerates lie between two rocks, they seem to have resulted from the motion of the parts on each other, in consequence of sudden and violent fractures, accompanied by a partial comminution of the materials. Where one rock alone has been engaged, a conglomerate of one ingredient, united by a general cement, is the result; and this case is frequent in the calcareous rocks. When the fractures have taken place at the meeting of two strata of different rocks, or when two have been in any other mode implicated, the compound is more intricate. Thus also there are formed conglomerates of limestone and serpentine, or of limestone and argillaceous schist, or of other substances.

There is yet one origin for the local conglomerates to be stated, which, if somewhat analogous, possesses a distinct interest. It may also be added as a superfluous proof which I did not think necessary to adduce, towards the posterior and forcible intrusion of trap and granite into the strata. Such conglomerates are often found at the places where these two latter rocks pass through, or interfere with, the stratified ones; and that I may quote an instance from foreign authors, and therefore free from the chance of bias on my own part, I shall refer to Canzacoli near Predazzo, where this appearance attends the junction of a mass, which is, at the same time, granite and trap, with the secondary strata. In our own island, the vicinity of Oban offers very extensive and obvious examples.

There is little now to be said respecting the formation of the unstratified rocks, which does not follow from the views of their origin formerly held out. Of their materials, we can only know that they are those which are also found in the stratified substances, and can only conjecture, indiscriminately, that they have been formed by the fusion of some or other of these. Differences in the proportions of the several earths are the only grounds of judgment; and thus it would be inferred that granite was the produce of gneiss, micaceous schist, quartz rock, and, ultimately, of argillaceous sandstones, and that the ordinary traps were the produce chiefly of the argillaceous substances, slate or shale. More particular evidences in confirmation of this opinion, will be found in the histories of trap and granite. But that trap and porphyry have actually been thus formed, is clearly proved by finding those rocks entangling fragments of schists, in which the gradual melting down of the materials is distinctly traced. The fact itself is detailed in its proper place.

Of Transitions among Rocks.

The last question respecting rocks that appears to require examination, relates to the transitions, real or imaginary, that take place between different kinds. Being formed, as we have seen, of so few substances, and possessing so many analogies among each other, such transitions ought to be expected. That they exist, is no reason for an hypothesis which has been maintained on this subject.

Because there is a gradation of a certain kind among gneiss, micaceous schist, and quartz rock, and because it is possible, by selecting particular specimens, to make that transition still more extensive, it has been argued that all these rocks originated at one time, from a common solution, and were therefore the results of a continued crystallization from a fluid gradually varying. They who have chosen to maintain this doctrine, have certainly derived from it great convenience; inasmuch as they have dispensed with the labour of investigating the differences of these rocks, or describing their characters and connexions. I know not what advantages are to be gained by thus restoring Geology to its original chaos; and as the question of watery crystallization has been sufficiently considered in other parts of this work, that subject may be dismissed.

Such transitions as do actually occur, may easily be accounted for in various ways. In the older strata, they may arise from proximity of position, in rocks that have been in a state of semifusion and that were formed of similar materials. Thus they are common between gneiss, micaceous schist, and quartz rock, accordingly as these approximate. Thus, by intermixture, occasional transitions may also happen between coarse argillaceous schist and quartz rock, or between the fine schist and gneiss. But these are rare and easily explained; nor is there any transition from limestone to any other rock. In the newer strata, it is equally easy to understand how they must happen, from the irregular succession of so small a number of materials, and how some uncertainty of composition must often take place at the point of change between different deposits. This subject is however noticed more particularly in the chapter on the Successions of Strata; and the particular transitions will be pointed out, where necessary, in the histories of individual rocks hereafter. It is to be feared that the imaginary value attached to these transitions, has arisen from the practice, far too general, of deducing conclusions respecting the order of nature, from that made by a mineralogist in his cabinet. Undoubtedly, a rich cabinet may be made to produce every transition which the most arduous theorist could devise; but he will have far mistaken the real objects of his geological pursuits, who shall make his drawer the type of Nature.

CHAP. XIII.

On the Destruction of Rocks.

If the interest of geological facts bears any proportion to their importance as they affect the condition of organized beings, there is none in the whole range of the science more calculated to attract attention than that which relates to the destruction of rocks, to the sure though tedious process by which they are converted into earth and soil. On this process, depends the very existence of all the races of terrestrial vegetables and animals. In the smallest fragment that falls from the precipice, in the ceaseless flow of the torrent and the river, in the summer's rain and the frosts of winter, the Geologist contemplates the agencies by which Nature renews and extends the animated surface of the earth; and, recurring to the commencement of these actions, beholds it a dreary waste of naked untenanted rock.

Of the chemical Agents which tend to destroy Rocks.

In enumerating the agents by which Nature operates her important purposes of demolition and destruction, if some shall appear insignificant in their power, or tedious in their effects, there are others, of which the results are rapid, important, and sensible to us where-ever we turn our eyes. Even the agency of those chemical causes which at first appears so feeble, is often highly efficacious in preparing the way for the action of mechanical powers of more acknowledged and obvious force. But it must be remembered that whether the process of destruction be slow or rapid, and though it may, to our limited views, often appear contemptible in its effects, it is a process that never ceases. However limited these actions may have been as to the past, their unintermitting continuance through a duration to which we can assign no limits, must produce effects which we should vainly attempt to measure by the small portion of that time which is bounded by our own experience. Yet it will be seen, that even within the short records of history, the changes which Nature thus effects, are no less extensive than remarkable.

If the mere solvent power of water on the earths is the most feeble of these agents, its action is still unquestionable, and must not be overlooked. That it does dissolve silica and lime both, has already been shown; and thus it may often loosen the bonds by which the more insoluble substances are united, so as to produce a greater effect of destruction than would result merely from its solvent power. It is so easy to trace its action on limestones, that instances of this nature need not be adduced. The surfaces of quartz rock which are exposed to rain, are often polished as if by a lapidary's wheel, and the peculiar roundness of the angles, here evinces the cause. The effect, however, in such cases as this, may fairly be taken as nothing; since, as far as direct solution is here concerned, scarcely eternity itself could be imagined capable of dissolving a mountain of this refractory material. But the looser aggregated rocks of this nature, or the common sandstones, give every where abundant proofs of its influence, in the corrosion they suffer on long exposure, even where most pure. When they contain calcareous particles, it is more easy to suppose that their failure has resulted from the solution of that ingredient; and, in either case, it is easy to see that a considerable disintegration of rock may take place, even when very small portions of it have been actually dissolved.

In Granite, this effect is often very sensible, and it is no where more easily observed than in Cornwall. Cavities, containing water in wet seasons, are very common in the granite of that country, and they are often of considerable dimensions, while they are excavated in forms so accurately curved as to resemble works of art. They are well known to those who have interested themselves in the antiquities of that county, by the name of rock basins: having been idly attributed to the favourite Druids of antiquaries.

It is easy to trace their progress from that which can contain but a single drop of water; and, as this enlarges, the work of destruction goes on in an accelerating ratio; while the fragments of quartz and felspar, remaining, serve to prove the nature of the cause. Whether, in this case, the quartz is acted on, or the felspar, or both, is uncertain; but that the effect does not arise from any action on ferruginous matter, is unquestionable, as it takes place equally where the felspar contains no iron.

It is probable that a more indirect action of water has a similar effect in producing the disintegration of rocks, independently of its chemical power upon the iron which they may contain. It was elsewhere shown, that all rocks contained water when deep in the earth, and that, so far, they are porous, however solid in a general sense. By being alternately wetted and dried at the surface, they may thus undergo alternate contraction and expansion, in such a degree as, in time, to lose their integrity, as far as that operation can reach; independently of the frequent solution and precipitation of their soluble parts which must thus take place.

The destruction of the calcareous rock of Malta, described by Dolomieu, may be noticed in this place, though it is a peculiar instance, not arising from water alone, and apparently limited to that rock. When wetted with sea water, a crust is soon formed, which exfoliates and is followed by others, till the whole stone falls to pieces. It is still more remarkable that a single drop is capable of producing this effect; and that it even extends through a whole wall, where the salt has not reached.

It must be remarked that those rocks which are subject to decompose in considerable masses, as if by an universal caries, even when situated deep in the earth, contain alkali as an ingredient in their composition. It is in granite, in gneiss, and in the trap rocks, that this circumstance is peculiarly frequent and remarkable; and, in all these, either potash or soda is found, in some, both. This effect, unless when it arises partly from changes occurring in the ferruginous ingredient, must probably be attributed to the solvent power of water; and, however difficult it might be imagined for water to disengage alkali from such a combination, the possibility is confirmed by the effect which it is known to produce on glass.

The changes which are experienced by iron, are however the most conspicuous, and apparently the most important of the chemical agencies by which the decomposition of rocks is effected. This metal exists in stones in different conditions, all of which, it is probable, have not yet been ascertained by chemical analysis. But as far as the present object is concerned, the subject is sufficiently understood.

In the state of peroxyde, in which it communicates a red or brown colour to rocks, iron is very little susceptible of further changes from the action of water, and, in general, it undergoes none. Hence stones of this colour rarely experience any alteration of hue on exposure; and, what is much more important to architects, they are, in general, less susceptible of decomposition than those which are dark or lead-coloured. The red sandstones which are subject to decomposition, owe that defect to the other causes above enumerated. The existence of this oxyde of iron in certain rocks, is often an interesting circumstance, as throwing light on the changes which they have undergone; but it is essential not to carry this speculation too far. The clays found under the trap rocks, appear unquestionably to owe their red colour to the action of heat. Similar appearances are, in some cases, observed in the argillaceous and micaceous schists; but it is not certain that, in all these, the same cause has acted. In the red sandstone, if it does, in some instances, appear to have produced the effect in question, the colour, in the majority of these, has been derived from that of the felspar which furnished the clay in their composition, or else from the original quartz.

The next state in which iron exists in rocks, is in that of the yellow, or the hydrated, carbonat; forming the rust which colours all the yellow rocks and clays. In this case, however, it is even less a source of decomposition than in the former, and is, indeed, in general, perhaps in all instances, itself the produce of decomposition. That some of the yellow rocks have been once lead-coloured, may be seen in many of the secondary limestones, where the change can be traced; but it is scarcely suspected that this process has been carried on in so extensive a manner in the sandstones, which I shall nevertheless hereafter show.

Iron, in the form of pyrites, is known to destroy many of the secondary sandstones in which it exists. But it is not sufficiently common to produce any extensive or conspicuous effects; and, in the state in which that mineral occurs in the primary rocks, it is scarcely susceptible of decomposition.

It is in the state of the diffused or combined protoxyde, that the power of iron in decomposing rocks is most remarkable ; and it is of sufficient importance to be numbered among the causes that are most active in the work of destruction. The chemical process is simple ; as this oxyde combines easily with water and carbonic acid, thus producing rust, which is indicated by the yellow and brown colours which the rocks that contain it acquire on exposure. It is probable that, in these cases, the actual decomposition is the consequence of the increase of bulk which the stone thus acquires ; and therefore, in many instances, where the quantity of this ingredient is small, the rock does not moulder, although it becomes more tender. That is an event not unfrequent in many limestones and sandstones ; as well as in some of the claystones of the trap family, which retain their integrity after their colour has been changed. Where, as in many basalts, the iron abounds, the event of this change is to resolve the whole into clay. There is yet however some obscurity in this subject, which it requires the future aid of chemistry to dispel. Many shales and slates, though apparently containing this oxyde, resist all change of colour, and, in consequence, preserve their tenacity when rocks of other kinds, of the same colours, fall to powder, or, at least, become softened.

It has not hitherto been suspected that the carbonat of iron was a common ingredient in rocks; and though its effects in accelerating their decomposition is very trifling, it is proper that it should be enumerated among the rest. It is frequent in the white veins of carbonat of lime that traverse the schists, and is detected by the brown colour which it acquires from the action of water. Its existence would as little be suspected in the white quartz rocks. Yet, in these also, it is discovered by their becoming rusty on exposure, sometimes to the depth of even a quarter of an inch; as, by the same means, it is ascertained to exist in some of the whitest compact felspars. It ought to be added, that the rust thus formed, appears to be soluble in the water of the atmosphere; as such rocks become bleached at the surface, while the brown stain occupies an inferior lamina. It is thus that we must account for the whiteness of the powdery surface so often found on the decomposing argillaceous schists and felspars, even where we are sure that these contain iron. If there are any other chemical causes for the decomposition of rocks, they are still unknown to us, and must remain for the investigation of future chemists.

Of the Decomposition of deep-seated Rocks.

Although the phenomena in question occur most conspicuously where rocks are exposed to the atmosphere, or to the action of air and water together, they also take place deep in the earth, as already remarked; in which cases they must be referred to the effects of water alone.

As some of these changes are, from their extent or other circumstances, of a very interesting nature, and may perhaps serve to explain some difficulties in geology, it will be necessary to bestow a few words on them.

This event is so common in granite, that examples of it must have occurred to every geologist. It is frequent in Cornwall and on the opposite coast of Britany; and the result is a clay mixed with quartz gravel, and, in some cases, where the mica has resisted decomposition, with that mineral also. In some kinds of gneiss, it is equally common; and remarkable examples of this nature may be observed in Aberdeenshire, in Guernsey, and in the isle of Sky. If less frequent in micaceous schist, it is still sufficiently conspicuous in that rock, in many parts of Scotland; as, in Cornwall, it is of very noted occurrence in the soft argillaceous schists, or the "killas," of that district. That the white, or pure, sandstones undergo this change in the same manner, is proved by the occurrence of a hill of considerable extent near Kildrummie in Aberdeenshire. This hill consists, almost entirely, of loose sand; and thus its original rocky state might be disputed, did not the permanence of numerous harder veins that intersect it, and the remaining marks of the joints which divide the beds, prove its true nature. It has often been doubted whether the sand beds in England, which contain sandstone and sand together, were in the progress of decomposition or induration; and it has, I believe, been generally decided in favour of the latter. The present fact may throw some doubt on the propriety of that decision, and will at least render it incumbent on geologists to search for evidence capable of determining with more precision what the truth is.

Such is the importance of this view, and so extensive the consequences to which it leads, that I shall not be disappointed if geologists should refuse their assent to it; though the facts next to be recorded are equally satisfactory, and are at least free from the suspicion of supporting any hypothesis.

The chief part of the Orkney islands consists of an alternating series of sandstone and shale, belonging to the lowest, or *old* red sandstone of geologists. In some places, the sandstone is red, but, like the shale, it is more generally of a dark gray colour. But there are also beds of a yellow and tender kind, exactly resembling some of those which occur among the upper secondary strata, and, at first, leading to a belief that such a series exists here. It is only after much examination that the true nature of this rock is discovered; when it is perceived to arise from a change in the iron of the blue strata, which thus become tender as they acquire the yellow colour. Acting on this hint, it will remain for geologists to inquire whether similar changes may not have taken place in many other cases, where the tender and yellow state is supposed to have been the original condition of the strata.

This deep decomposition is frequent in the trap rocks; and, in some of these, it leads to important practical consequences, while it gives rise to geological suspicions of no small interest. The very deep and rich soil of some parts of Scotland, which lies above sandstone, is evidently derived from this source, and, apparently, from an entire resolution of the compact trap that has once covered it. In Sky, the same fact is presented in a manner perfectly unquestionable. In consequence of the protection afforded to the subjacent rock by the solid mass of peat with which it is covered, it remains so undisturbed as to present all its divisions and concretionary forms as if still in a state of integrity; the zeolites which it contains remaining also unaltered. But it is no longer a rock: the spade and pickaxe cut through it as through earth; and, where natural forces have acted, the whole moulders into yellow clay, leaving the zeolites in heaps resembling banks of gravel.

In Bute, there are found beds of a tenacious compact clay, lying deep under a mass of solid trap, and presenting what, on a superficial view, would be deemed a natural and original state. On an accurate examination, however, it is discovered that they are interspersed with crystals of felspar, sometimes entire, but more frequently reduced also to clay, yet of a different colour from that which forms the base. By this and some other appearances, it is proved, that these have once been masses of porphyry, which have thus undergone decomposition, deep in the earth, while the rocks above them retain their original integrity.

In the islands of Luing and Torsa, there are some large veins, consisting, apparently, of that yellow arenaceous claystone so well known to the geologists who have visited Arran; and, in some places, they present a porphyritic structure. No suspicion respecting these could have arisen, had it not been for some deep and fresh fractures.

Thus it is discovered that the original rock is a dark compact claystone, often called basalt, in some parts porphyritic; and that the yellow claystone is not a natural rock, but the consequence of an incipient decomposition, the progress of which is thus easily traced. This rock, it must be observed, is perfectly compact and tenacious, although far less so than that from which it has been derived. In many of these instances, where the rocks are of a porphyritic character, the fact of such a partial decomposition may be suspected, from the existence of cavities containing yellow or brown clay; and, even in porphyries, of which the base appears to have undergone no change, the crystals are sometimes reduced to powder.

That the compact clinkstones may be converted into arenaceous claystones by this kind of decomposition, may be proved by the state of some of the rocks in Arran; where the former are frequently found covered with a crust of the latter, and where, without fracture, it would not be suspected that the rock was of a different character within. The same circumstance is visible in many places in Sky; and, from the sections there afforded, the decomposition of these rocks can often be traced to an enormous depth. It even appears that many of the pale and yellow Syenites and porphyries of that island have been originally blue; and that their imperfect compactness, like their yellow colour, is the consequence of incipient or imperfect decomposition. I may confirm my own remarks by the testimony of the French geologists respecting the rocks of Domfront in Britany; where greenstones are similarly found converted into claystones, which, by a further decomposition, are resolved into a species of fullers' earth.

So great is the depth of the decomposition in some of these instances, and so exactly do many of the great masses of pale claystone correspond with those which can be proved to result from decomposition, that we are led to suspect that this may be the origin of the whole, and that even all the rocks of this character which occur in Arran and Sky, may once have been clinkstones or compact blue claystones. The very same phenomena occur among volcanic rocks; and, in both, many of the substances to which the unmeaning term *trachyte* has been applied, are only the produce of this change.

It is not safe to carry this speculation too far; but it is plain that it opens a way towards the explanation of many circumstances in the history of the trap family, which have been hitherto matters of difficulty. It is not easy to imagine, for example, that rocks so earthy and loose as some of the claystones are, were the produce of igneous fusion; but since such masses of the harder rocks can undergo that change of character which has been described, this difficulty ceases. In the same manner, it has often been objected to the igneous theory of trap, that it is inconsistent with the presence of clay or unconsolidated rocks in the subjacent positions in which they sometimes occur. The case described as existing in Bute, solves this difficulty; as it will explain many others where similar anomalies have been found to exist.

Of the mechanical Agents which tend to destroy Rocks.

The mechanical causes which operate in effecting the destruction of rocks may act, either on masses already decomposed, or on rocks which have undergone no change; but it is unnecessary to distinguish the two cases. The most universal of these is friction, which is here enumerated in the first place, although not always called into action till the larger parts have been separated by other causes. But it is water which is here the moving force. Aided by the power of gravity, or urged by the violence of the winds, it impels against each other and against the solid rocks, those fragments which have been detached; reducing them to powder, or to sand and clay, as far as its power extends; and, where that has been exhausted, leaving the larger fragments at rest on the plains or on the bottom of the ocean. The efforts of this power are most conspicuous in mountainous countries, where the agents may be seen at work in the bed of every torrent; but if we would look for the effects which it produces, we must search the plains, the rivers, and the bottom of the sea. It is here that nature accumulates the collected labour of ages in one spot, as evidences of the power of that element which the geologist must never forget,—Time.

It is easy to examine, even the smallest effects of friction, in the rounding of the stones found in the beds of rivers, and in the sand and clay which they deposit. Near the sources of the torrents, the fragments of rocks are angular and the waters are clear, except where they may invade a soil already decomposed. As we proceed along their courses, marks of wear are perceived in the stones which they hurry along: by degrees they become rounded, gravel and sand are intermixed with them, and, as at length we reach the plains, the finest particles alone are suspended in the shape of clay; being deposited along its course as the velocity slackens and as gravity may direct; and, at length, as they outlive these actions, carried into the ocean, thence never to return again in the same forms.

The marks of this force are no less visible on the solid rocks; in the deep furrows which are every where to be seen in the beds of rivers, produced by the incessant friction of the heavy bodies carried along their surfaces. These furrows, it is well known, are found in places where rivers no longer flow, and they have been supposed the marks of antient deluges. In many of the instances that have been adduced, they are clearly the effects of rivers which have changed their places; but as there is much that depends on this question, unconnected with the present inquiry, it needs not be agitated at present.

As the accumulated products of friction are best examined in the plains and on the sea shores, so its effects on the solid rocks may most advantageously be witnessed in the deep sections which the torrents produce in the mountains. That these are really the effects of friction, is proved by the accurate correspondence of the rocks on the opposite sides. But no where is it more clearly evinced than in the falls of rivers, where these forces act with increased energy; the traces of cascades being frequently found far remote from the present places of the falls, and preserving the most impressive records of all that has been destroyed.

But it is not only in rivers that the power of water is exerted in causing the hard materials of the earth to contribute to each other's destruction. The fragments which rains and frosts have separated from the cliffs of the sea shore, rolled without ceasing by the efforts of the waves and the tides, are gradually reduced to powder; forming beds of sand and mud, and contributing, during their own destruction, to that of the solid rocks against which they are incessantly impelled. But the great destruction of the sea cliffs, that of which the traces are so strongly recorded in the outlines of all rocky coasts, is produced by causes of a far more active nature, which it is next my business to consider.

If in contemplating the towering peaks and the solid precipices of an alpine region, braving the fury of the elements and the floods of winter, the spectator is at first impressed with the character of strength and solidity which nature here seems to have conferred on her works, it requires but a moment's reflection to show, that every thing around him bears the marks of ruin and decay. Here he learns to withhold his regret at the perishable nature of all human labours, at the fall of the strong tower and the solid pyramid, when he sees that the most massive rocks, those mountains which seem calculated for eternal duration, bear alike the marks of vicissitude and the traces of ruin. Gravity is here the great agent in those changes which most forcibly arrest his attention; changes by which the solid precipices are shivered into atoms and hurried into the valleys beneath.

In these great revolutions however, other agents must co-operate; and the first here to be considered is the power of frost. Expanding as it freezes, the water which has entered the fissures, acts with irresistible force, and detaches those enormous masses which, in the seasons of winter and spring, daily fall from the mountains. In Greenland, it is said that these effects often take place with a noise emulating thunder; but, if less conspicuous, they are sufficiently common in all the alpine regions that are subject to the extreme vicissitudes of heat and cold.

It is to causes of this nature that the great ruin of the sea cliffs is to be attributed, and not to the force of the stones which the tide impels against their bases, as has been sometimes asserted. No stronger proof of this is required than that which is open to inspection on the shores of our own island. In the Western Islands of Scotland, it is often rendered very conspicuous by the prolonged basements and ledges which skirt the cliffs, rising precisely to the level of high water. This effect is exceedingly remarkable in Sky, and in the neighbouring islands; where it is not limited to any class of rock, although most conspicuously exhibited in the trap family. Thus we find towers and pinnacles rising out of the sea, pitched on a wide base below it, and waiting for the day which is to level alike the whole structure. Hence the approach to these rugged and dangerous shores is rendered impracticable by the long ledges of rock which skirt them, on which a formidable surf is for ever breaking. Hence also those sunk rocks, the terror of the mariner, and the remains of those which once towered above the water, but are now at length secured from further destruction. Thus, as the atmosphere destroys, the sea protects, for a time at least, from further injury; preserving, in these monuments, the most impressive records of what the land once was. In Arran, the long piers, the remains of trap veins, which stretch out into the sea on all sides, thus serve to mark the former extent of the less durable land which has at length submitted to that power from which even these records will not for ever be exempted.

It is not however to frost alone that we must attribute those enormous masses of ruin which so often fall in alpine regions, burying the plains together with their inhabitants, and, in our own diminutive Alps, causing those slides of large portions of the hills which may be seen in numerous places. In these slides, the geologist may often study the diminutive copies of those far greater movements which have formerly involved whole countries; producing the dislocations of the strata, and presenting, at their places of fracture, the models of mineral veins.

The exact causes of these great revolutions have not always been ascertained; but they have been attributed, in many cases, and apparently with justice, to the action of water. In other instances, they appear to have resulted merely from the effects of gravity, acting through a long period, on masses, of which the support was gradually becoming enfeebled; till, the powers of resistance yielding to those of motion, the whole has given way. A few of the instances recorded by authors may amuse the reader, and diminish the dulness of continued geological discussion.

In 1618, the town of Pleurs, near Chavennes, containing 2000 inhabitants, was suddenly overwhelmed, and the ground is now covered with houses and cultivation.

Near Passy, between Salenches and Servoz, a mountain thus gave way in 1751, with such a tremendous concussion, followed by clouds of dust, that it was supposed to have been the effects of a volcanic eruption. The examination made of it by Donati, renders this example interesting, as the cause was ascertained. It was found that the water of some lakes which were situated above, had insinuated itself between the strata of schist and limestone of which the mass was composed; and that its support having thus been removed, it had slidden and fallen. The mass of matter thus displaced was estimated at 3,000,000 of cubic fathoms, and was sufficient to form a considerable hill.

But the most recent occurrence of this nature was in 1806, and the fall was so sudden as to overwhelm nine out of thirteen persons who were travelling near it at the time. This fall took place in the Rosenberg; detaching the summit called the Knippenhoul, together with a portion of the adjoining ground. Falling into the valley which separates the Lake of Zug from that of Lawertz, and into the latter lake, it produced an inundation which caused a great destruction among the houses and the population; obliterating at the same time a large portion of the lake. The plain is now covered by a hill, a hundred feet in height, and a league and half in length and breadth. In this case also, the cause was of the same nature, originating in a lake situated above Spietsfleu.

It is unnecessary to describe the minor events of the same nature which have occurred in our own country, as the causes have not always been so well ascertained. But it ought to be added, that, in the Alps, great devastation is sometimes produced by the sudden eruption of lakes, caused by the wasting and bursting of their barriers; instances of which have been noticed by De Luc and by Saussure.

In terminating this subject, it must now be remarked, that the progress of disintegration and decomposition is resisted by vegetation, and by the accumulation of alluvial soil; as it further is, in the lower lands, by the diminution of the power of gravity, and by the gradually diminishing ratio of the other active powers of destruction. Thus it has been remarked by Dolomieu, that, in the Lipari isles, the volcanic fragments are first arrested by the vegetation of shrubby plants, until, finally consolidated by a more minute vegetation, they become a firm and permanent covering to the mountains. In a similar manner, in Sky, the dense coat of peat, impermeable alike to air and water, protects from further waste, that decomposed trap which would otherwise shortly be hurried into the sea by the rains of this watery climate. But here, that which might benefit may also injure; as the rocks capable of producing fertile soils when exposed, such as the limestones and traps, are thus excluded from the reach of destructive but useful agents, till the labour of man learns to cooperate with the designs of nature.

In all these operations, we trace the beneficent hand of Nature, which, by an admirable counterpoise of the causes of ruin, supplies from the higher lands that which the daily operations of the rains are removing from the lower. Wherever soil is removed, it becomes thus replaced; and, if not from the higher lands, it is renewed by means of the access which the elements obtain to the rocks beneath, in consequence of the removal of their protecting covering. Thus also, where the destruction is greatest, the supply becomes proportionally rapid; preserving that state of perpetual youth which, under whatever changes, is still present in all the works of Nature's hand.

Thus far the processes of destruction, or the decomposition and the disintegration of rocks, have been considered under their simplest modes, and as they relate to the immediate effects which follow them, namely, the demolition of the solid land, the formation of soils, and the deposition of loose materials in new situations. It remains to examine some peculiarities in the process of decomposition, which are interesting to the geologist in another point of view; being remarkable, partly from their singularity and the difficultyof explaining them, and partly from discovering to us some peculiarities in the concretionary structure of rocks, that would not otherwise be conjectured.

Of the Desquamation of Rocks.

It is well known that many rocks of the trap family, undergo a process of desquamation after a long exposure to air, and are thus gradually resolved into crusts, which continue to fall off in succession, at length mouldering into clay. The same appearance, although more rarely, occurs also in granite; and, in both cases, it has been conceived to depend on an internal concretionary structure, and to indicate the mode in which the constituent parts of the rock are arranged, which, however invisible in the fresh fracture, is thus rendered evident by the progress of decomposition. In the case of the columnar traps, whether basalt or greenstone, this desquamation often proceeds in such a manner, from the circumference of a joint towards the centre, that the result is a spheroidal body.

This effect, compared with the spheroidal concretionary structure which is known to take place in basalt artificially fused, has appeared sufficient to justify the general conclusion that all appearances of a similar nature depend on the same cause; and, by a slight addition, it has been held sufficient also to account for the jointed and columnar structure of the rock in which it occurs. The result of a more careful examination has been to prove, that two causes, perfectly distinct from each other, operate in producing the same effects; and a detail of the facts in question will not only serve as a caution against the universal adoption of a well-known rule in philosophizing, but to record an interesting circumstance in the history of the rocks in which these appearances have been observed.

Of the Desquamation of Granite.

In some columns of red granite brought from Leptis in Africa and lately in the British museum, the shafts are in the act of desquamation; casting off crusts similar to those which are occasionally seen in natural blocks of granite, and equally resembling, except in their superior integrity, those which are found on the surfaces of the columnar trap rocks after exposure to the weather. It is obvious that the form of the shafts can bear no relation to the original forms of the blocks of granite from which they had been wrought; and it necessarily follows, that this desquamation can not depend on an internal concretionary structure, but must have resulted from the action of the weather on the exposed surfaces.

It is worthy of remark, that, in this case, the detached crust is not decomposed, and that, except in tenderness and fragility, it appears scarcely changed from its natural state, Nor is any stratum of clay or decomposed matter found at the place where the crust separates from the solid block; at least none such was found in the places which I had an opportunity of examining.

The decomposition of rocks has in most cases, and with justice, been attributed to changes in the state of the iron entering into them; but it is evident, that neither this, nor the other causes which have been supposed to produce that effect, are capable of explaining the very singular process in question; as it is not conceivable how the ordinary action of the atmosphere should affect the interior part of the stone, while the surface more immediately exposed to its agency has escaped. Now, in examining with attention those cases in nature where the same process takes place on the exposed surfaces, it will be found that the desquamation of the prismatic or cuboidal masses of granite which are susceptible of this change, takes place all round the surface, respecting some imaginary point or centre, and promising, in the progress of time, to reduce the whole to a smaller and more spheroidal mass. Hence no conclusion can be drawn as to the cause; since the desquamation may, in this case, be either the result of an internal concretionary and laminar structure respecting one centre, or the consequence of a process similar to that occurring in the columns of Leptis.

But in examining other cases of desquamation in granite, a different appearance will be observed. In these, it may be seen that a single block desquamates in a manner so complicated that no parallelism is maintained between the surfaces of the stones and the crusts; and as, in some cases, such blocks are so thoroughly softened as to admit of being cut by a spade, it is not difficult to discover that more than one, or even two centres of desquamation, exist in a single mass; the surfaces of the different spheroids interfering and compressing each other where they come into contact; and the intermediate parts, which are still required to fill up the solid, consisting of deficient portions of crusts, respecting one or other of the approximate, imbedded or internal spheroids. It is evident that, in these instances, the effect could not have resulted from the action which produced the crusts on the columns just described; but that it must have been determined by other causes, depending on an interior structure, the existence of which has already been proved. Thus, two distinct causes act in producing the desquamation of granite; and as, in cuboidal blocks, it cannot be determined to which of these the effect must be assigned, the same difficulty often occurs in those cases where the desquamation takes place in straight laminæ. This is the case of schistose granite, as it has been termed.

The island of Arran affords very accessible examples of this, and it is disposed, most generally, in extended laminæ of large dimensions, but is also occasionally prismatic. The forcible fracture of these blocks does not detect the slightest indication of a laminar structure; and there is no foliated disposition in the integrant parts of the rock, which can account for this desquamation; as it does not bear the slightest resemblance, even to granitic gneiss.

The detached laminæ are not less tenacious than artificial ones of the same thickness would be; appearing indeed, in every respect, perfectly natural. Neither do their surfaces exhibit any signs of decomposition; being, on the contrary, brilliant and clean, as if cut by art. The same cleanness and freshness of both the surfaces in contact, are found where the laminæ and the block are separated; nor is there any loose matter generated, nor any appearances of decomposition in the plane which disjoins them. The thickness of the laminæ varies from one eighth to a quarter of an inch, but they are seldom uniform, in this respect, throughout.

From the nature of the desquamating crusts, it might be supposed that this case resembled that of the columns; but, in this granite, the desquamation takes place only on that surface which is parallel to the chief planes of the great laminæ of the rock, and not on the sides of the prisms; whence it is probable that the great concretionary structure which formed the large laminæ, is also the cause which influenced the desquamation of the small one.

Of the Desquamation of Trap.

It will next be seen, that although some of the examples of this occurrence in the trap rocks, are truly dependent on the concretionary structure, others are as unquestionably the result of actions similar to those which produce the desquamation in artificial blocks of granite. Thus they confirm the views, already held out, of one common effect proceeding from two causes; while they also offer another analogy, to add to the numerous resemblances which exist between granite and the rocks of the Trap family.

In the columnar traps, whether basalt or greenstone, the crusts fall off in succession, in such a manner that the angular or prismatic form at length disappears; and the ultimate result is therefore a spheroidal body, destined finally to be also resolved into clay. It most frequently happens, that as the crusts are separated, they also fall to pieces, or become loose clay; but, occasionally, they retain a considerable degree of tenacity; although, by the changes of their colour from the natural dark blue to brown, it is evident that the iron has undergone a chemical alteration; being converted from protoxyde to rust.

Where similar rocks possess a rude and imperfect prismatic structure, the same effects also take place; the surfaces of the prisms desquamating in such a manner as to leave a congeries of spheroidal or ellipsoidal bodies, destined, in the same way, to be ultimately resolved into loose earth. But these changes are not limited to prismatic trap only, since they occur in other and different forms; being attended with the same final result, whatever the shape of the block may have been, namely, the ultimate production of spheroids.

Although, in some of these instances, it might be imagined that the effect of desquamation was produced, as in the case of the artificial granite columns, by the exposure of the surfaces to the air, it will, I believe, be found that, in nearly all, they truly depend on an internal concretionary structure.

The chief argument for this opinion will be found to consist in the effects which take place in those traps that are not jointed, as well as in those masses which affect a prismatic fracture without being absolutely divided into prismatic forms. In these, as in some of the cases of exfoliating granite, the desquamation is of a complicated nature, referring to more than one centre. Thus, in a single unjointed prism, the same result takes place as in those with joints; numerous spheroids being discovered in its length, resulting from the progress of desquamation. The same effect is produced in those irregular masses which are characterized merely by a prismatic fracture; as the exfoliation commences in many different places, referable to different central points, so as to leave, in the same way, a number of spheroidal bodies imbedded in a mass of loose crusts and clay. It is further, indeed, often to be observed, that in cuboidal or otherwise irregular blocks which have neither prismatic form nor tendency, there are several centres of exfoliation: numerous balls being thus finally extricated from a single solid block, of which all the surfaces are equally exposed to the action or contact of the atmosphere.

It must now be observed on the other hand, that masses and fragments of trap, which have received their forms by art or accident, sometimes show the same tendency to exfoliation on all the exposed surfaces; into whatever forms they may have been broken, and whatever their size may be. In the smaller fragments, the process has sometimes been carried so far as to leave solid balls covered with a succession of crusts easily detached. It is plain that, in the case of irregular fragments so formed, no concretionary structure can be suspected; as it is not within the limits of possibility that they should have been broken from the larger masses, in a fortuitous manner, and so that the centre of the fragments should have coincided with a concretionary centre.

It will render this subject more complete, to extend this inquiry to the case of the schistose, or laminar trap; a subject which has either been neglected or much misapprehended.

The tendency to flat laminar exfoliation on the surfaces, is more common in the rocks of this extensive family, than the spheroidal, since it occurs in every species that I have examined; whereas the latter is rarely found except in basalts and greenstones. The internal flat laminar structure which is independent of the agency of the atmosphere, seems also to exist in a greater number of species than the spheroidal concretionary form does.

This species of exfoliation of the surface of trap, occurs in so many parts of the Western Islands of Scotland, that it is unnecessary to particularize the instances. In one or two, it is found in a claystone of a columnar form; the exfoliation being at right angles to the axis of the prism. There are rarely more than the indications of three or four divisions; not above one or two of these being so far separated as to admit of being removed, and the deeper parts of the rock remaining unaffected, so that this structure cannot be inferred to pervade the whole. As the irregular rocks are much more common than the columnar, the instances of schistose exfoliation are, in these, similarly, more frequent.

Among the different kinds, the only one which presents a large laminar disposition analogous to that of granite, is Hypersthene rock. In this, the exfoliation sometimes occurs on the surface; but it is always limited to one scale or lamina, which, in its thickness and tenacity, and in the general appearance at the plane of separation, exactly resembles that which is produced in the schistose granites.

Few marks of such a large laminar disposition are to be observed in the Syenites, claystones, and clinkstones, or in the porphyries derived from the two latter rocks; though Mull presents some remarkable exceptions. No relation can therefore be inferred between the direction of the exfoliation and that of a larger mass; and accordingly, we can only conclude that, whatever surface is thus found exfoliating, the direction of this change relates to that of the exposed surface. Yet as these rocks seldom exfoliate in two directions at any one place, it is probable that this tendency does actually bear some relation to the internal structure of a mass, though no other indications of it are visible. That such a structure does occur in many traps, is an argument in favour of this opinion.

The number of successive laminæ which may be detached from any of the rocks last named, is various; but I know not that it bears any constant relation to the species. I have observed only, that when they do occur, they are more numerous in the softer claystones than in the Syenites, or in the rocks with a base of clinkstone. If one, two or three, can be detached in succession, that facility soon ceases; and, after some partial indications of future desquamation, the rock is found to be massive, and to give no marks of the future renewal of a similar process; though there can be little doubt that it will continue in succession, as the preceding laminæ become detached. Such laminæ generally present the same average, but variable thickness, already mentioned. The surfaces are also occasionally undulated or irregular, and, in a few instances, slightly curved. From preserving the tenacity of stone, it is generally supposed that they are unchanged, and that they present the natural characters of the rock to which they belong. But this is not the fact; and they will be found, in this important circumstance, to differ essentially from those laminæ which pervade the interior of veins, and which arise from an internal concretionary structure.

The deception has, in these cases, arisen from the great depth already noticed, to which the rocks of this family are sometimes affected by the weather without losing their tenacity; undergoing little apparent change but that of colour. Hence the solid parts of such rocks, when broken, even below the exfoliating surfaces, will be found to present the same appearance as the detached scales. But if a deeper section be made, it will, I believe, invariably be observed, that the natural state of the rock is different, and that the process of exfoliation has arisen from a partial decomposition. To a misconception of the nature of this phenomenon, we are indebted for the very improper term Porphyry slate, and the much more improper distinction.

Of the Desquamation of micaceous Schist.

Whatever difficulties may appear to exist in explaining these appearances, they are exceeded by a similar one in Micaceous schist; although, but one example of this, has occurred during my researches. It is in the large block known by the name of Ossian's tomb, which lies in Glen Almond.

Although this stone bears, on all the surfaces, those slight marks of decomposition so well known in micaceous schist, which consists in the rusting of the iron in the mica, it is only on one side that the peculiar effect in question has taken place; being that on which the wind and rain beat with most violence. From nearly the whole of this surface, scales of the rock can be detached; scarcely differing in tenacity and hardness from the original stone, and thus resembling those which exfoliate from the granite columns. Their thickness varies in different parts, from the sixth of an inch to nearly the half; and they may be obtained in plates of considerable extent. At the planes where they separate from the mass of the rock, no marks of decomposition are visible; both the surfaces being clean and smooth, as if cut by a sharp tool.

There is here also a succession of similar operations visible. At some former period, the plates first detached have in some places, fallen off, so as to leave a new surface exposed; and, on this, the same desquamation has again taken place, so that, in some parts, two successive plates can be separated. In other places, it is apparent that the progress is about to be completed at some future period; the edge of the scale just admitting a knife, but it being as yet possible to detach only a small portion at the margin. Sometimes, there is even an indication of a third scale: leaving no doubt that, in a sufficient length of time, the same process may be expected to take place through the whole block, should the same external circumstances continue to act.

This fact would have been in no respect remarkable, if the desquamation had taken place in a direction parallel to the laminar structure of the stone. But it occurs at considerable angles to that, so as to produce a scale or slate, which consists of parallel bands of quartz and mica: both of them remaining unchanged, as in the solid rock, but easily separated, in consequence of the fragility of the micaceous band. It is evident therefore, that in this instance, the desquamation is, not only, not produced by the peculiar structure of the rock, but is utterly independent of it. Under the same circumstances, it might equally be expected to occur, either in a mass of pure quartz rock, or in a micaceous schist of a more simple and homogeneous nature; and, in this latter case, the schist might desquamate at angles to its fissile tendency. Such an occurrence would excite surprise; but there is no apparent reason why it might not happen in either of these rocks in a separate state, as it does here where they are intermixed in distinct laminæ.

The peculiar circumstances of predominant exposure to the weather on the one side of this rock where the desquamation takes place, contribute to prove, still more clearly than in many of the instances formerly enumerated, that the whole of this process is caused by the action of the atmosphere and the rains. However mysterious it may at present appear, it is the result of some chemical agencies which cannot for ever be concealed. How far it may be connected with any circumstances of the same nature more generally interesting, it is impossible to foresee. But like all new facts in an obscure science, it is worthy of record. In multiplying the examples of difficulties and obscurities, they become gradually removed from the list of exceptions; while the varieties which are discovered in them on the comparison of many examples, sometimes point out the causes which have influenced the whole.

Of some peculiar Modes of Decomposition in the Rocks that have a venous and cavernous Structure.

It was formerly remarked, when on the subject of internal structure in rocks, that the existence of a venous and reticulating arrangement in the parts of granite and of some other rocks, was detected only by the consequences of decomposition. This appearance is more common and more easily observed in granite than in any other rock, but it also occurs in gneiss and micaceous schist. Wherever it happens, the surfaces exposed, either to the weather alone, or to that imperceptible friction which is caused by the tread of animals, the motion of water, or other slighter actions, are corroded into shallow and unequal cavities; the boundaries and forms of which are determined by the casual reticulations of the harder and more refractory veins by which the rock is intersected. In many places in Aberdeenshire, where the whole mass of granite has decomposed together, in the manner hereafter described, the permanence of these veins produces very remarkable appearances. While all the surrounding rock is converted into clay and gravel, the veins remain entire, and may be removed in solid pieces of considerable dimensions resembling slates.

The oval or oblong cavities which are occasionally seen in many of the stratified rocks, and which sometimes strongly resemble the human footstep, seem to belong to the same cause. A most remarkable instance of these is seen at the entrance of Loch Craignish on the west coast of Scotland, where a double and alternating row is prolonged for some distance, in a variety of chlorite schist, in a manner so accurate as to represent a series of the foot-prints of an individual. In limestone, similar marks are familiar, and they also occur in clay slate, in Galloway.

The decomposition of the secondary sandstones is often attended by a great number of extraordinary appearances besides that above named; the consequences of internal structures which could not be suspected, and of which the fracture of the unaltered rock gives no indications. The spheroids of Egg, often reaching to three feet in diameter, are discovered only by the crumbling of the including portions; and, in Arran, a minute structure of the same nature is found, covering the surfaces which are exposed to the wash of the sea. On the shores of Fife, and elsewhere, the same process detects the existence of small cylindrical bodies placed in parallel order at right angles to the planes of the beds; as if colonies of the Lumbricus marinus had been petrified in their holes during the same process which converted the sands of former sea shores to stone. The very unaccountable and regular schistose structure of the sandstone of Strathaird in Sky, is equally invisible on breaking the fresh rock. But where it is exposed to the action of the sea, or to that of the winds and rains, the edges of the more durable laminæ protrude in regular relief; covering the faces of the cliffs with a continuous architectural ornament. It is worthy of remark that, in this instance, the influence of the associated trap, which has in some places indurated the original rock to the total destruction of its natural characters, has not succeeded in obliterating this peculiar structure; and if the grooves in the natural rock are deep enough to receive the hand, their appearance in the altered one is rendered only more artificial by their resemblance in depth and form to the finest flutings of the Corinthian column.

But the capricious resemblances to architectural decoration which sometimes occur during the wasting of these sandstones, are no less various, than they are ornamental when they are found in the stone which has already entered into the structure of a building. Our antient castles, in many places, will furnish examples of this nature which cannot have escaped the notice of the artist and antiquary, more than that of the Geologist. So fortunately are they sometimes placed, and so much do they conduce to the ornament of the walls where they exist, that it is difficult at first sight to avoid imagining that they have formed part of the architect's design, and are not actually the hatchings and vermiculations, the rustic work of the mason's chisel. Nature here sports in emulating the works of art, if indeed art has not in this instance borrowed from Nature. They who have amused themselves in tracing the origin of the Corinthian capital to the casual coincidence of a plant and a basket, will find even less difficulty in deducing from the natural wasting of sandstones, those rustic basements which have never yet equalled it in variety and artifice of design.

CHAP. XIV.

On the particular Order of Succession among Rocks.

It had been so often and so confidently said that a definite and constant order of succession existed throughout all rocks, that it had passed into an axiom in geology. Time has not yet dissipated this phantom, though it is gradually fading from among the realities in which the science abounds. As there are few among the dogmas of geologists which have more contributed to impede the progress of investigation, it will be useful to examine the grounds on which it still holds its place. The first step in forming a firm foundation is to remove the tottering materials of the old one.

As the doctrine of *universal formations* is in a great measure implicated in this hypothesis, the same examination will serve to try the truth of both. Such also are the catenations of hypotheses, that I must equally notice that branch of the same theory, which asserts that the successive rocks are found to terminate at lower levels above the mean surface of the earth, in an order corresponding to that of their superposition or formation.

As also it is held to be unfair to examine part of a theory separated from its connexions with the remainder, it must be remembered that in this one it is asserted that no extensive denudations of the surface have taken place, particularly in antient times.

In strictness of meaning, the term *universal forma-*

tions implies that every rock, from the lowest granite to the highest trap, surrounds the whole globe. The fresh water deposits may be overlooked in favour of this theory, as they have been discovered since it was promulgated. Every rock from granite upwards, ought therefore to be found in every place; unless that branch of the general theory is abandoned, which denies an extensive waste and removal of the superficial rocks. Thus this hypothesis is at variance with facts, at the very outset; since, whatever identical or analogous rocks may exist extensively in many parts of the world, no one is universally continuous. If the term *universal formations* means only that the same rocks occur in many different places, it does not fulfil what it professes, and the term of general analogies would better express its real meaning. But if every advantage be given, by admitting the doctrine of the waste and removal of rocks from the surface, it is then only necessary that the same rocks should once have surrounded the entire earth. Hence, wherever any series of similar strata exists in two places, they should be found in the same order, and no interior stratum should in any place be absent. That this is not the fact, will be fully shown in the subsequent remarks on the successions of rocks; and thus the doctrine in question is proved to be in every way unfounded. In examining the assertion that the levels of the most recent rocks diminish gradually in absolute height, in the direct order of their posteriority or superposition, it is sufficient barely to mention that Granite, which forms the summits of Mont Blanc, is found on the sea shores of England, and that the limestone of the same shores is found on the Jura. In the same way conchiferous limestone occurs at 14,000 feet above the level of the sea in Peru, and gneiss occupies the lowest tracts of Scotland and the Baltic. But the history of the successions of rocks, as they actually exist, require some further detail.

It was once universally believed that no stratified rock existed below granite; but if this substance is to be defined by its mineral composition, that opinion is unfounded, since I have already remarked that such a compound lies above conchiferous limestone. Nevertheless, in the arrangement of rocks which I have adopted, founded chiefly on geological position, the original opinion is retained; an arrangement that will not be objectionable to those who maintain the doctrine of fixed successions and the universal inferiority of granite.

Of the general Successions of Rocks.

It is unnecessary to commence these remarks by detailing the imaginary order of succession formerly received. Were it necessary to multiply examples of irregularity, it would be easy to refer to the writings of numerous geologists. They who are inclined, may consult the remarks of Ebel, of Breislak, and others. I shall here limit myself, chiefly for the convenience of British readers, to illustrations drawn from our own country, and, for the accuracy of which, myself and other British geologists must of course be responsible.

Granite is succeeded by gneiss very generally in Aberdeenshire, as it has been supposed necessarily to be everywhere. In the same country, it is followed by micaceous schist, as it is in Arran, in Upper Lorn, and in many other places. It is perhaps of little moment to say that it is often succeeded by hornblende schist, as this rock is noted for its versatility of place; but in various parts of Perthshire, and of the mountainous districts of Aberdeenshire, it is followed by quartz rock, as it is also by primary limestone. The immediate contact of argillaceous schist with granite in Cornwall, is matter of general notoriety; and the same sequence is found in Arran, in the Isle of Mann, and in Aberdeenshire. [illegible] II 316

It may also be remarked that, in many cases, there is merely an irregularity in the order of succession, while, in others, one or more out of the more common number of the strata are absent. In the last instance, for example, no other primary stratum than the argillaceous schist, intervenes between the granite and the more recent ones.

But cases also occur, in which all the primary strata are wanting.

In Aberdeenshire, granite is found to be immediately Vo. II. 91,
followed by the lowest sandstone of the secondary series; x Old red
and the same succession is found, on a very extensive scale, on the eastern shore of Caithness and Sutherland. It is still more remarkable, that, in the same neighbourhood, there reposes immediately on granite, -287 a series containing coal, which is generally separated from it by a long succession of intervening strata, both primary and secondary; being that of the oolithe and Lias.

The foundation of a theory of regular succession could not well be more defective; but it will be proper to examine the superstructure a little. If it be common to find gneiss succeeded by micaceous schist, and that by argillaceous, the exceptions are so numerous that the rule is of no value. In Perthshire, gneiss is not only succeeded by quartz rock, but alternates with it in endless succession; while, in other places, as in Shetland, it is also found forming a part of more than one extensive series, in alternation with quartz rock, and with argillaceous, micaceous, hornblende, and chlorite schists; to omit all notice of serpentine and diallage rock. It is regularly succeeded by argillaceous schist in the islands which skirt North Uist; and they are found together in Iona, in different parts of Rossshire, and in Sutherland. But to cut short an enumeration which it is useless to prolong, gneiss is immediately followed by the old red sandstone through a very extensive tract in Invernessshire and the neighbouring country; and, in Morven, by a series resembling that of Sutherland, consisting chiefly of Lias and its lignites, or coal, to the exclusion of all the intermediate substances. Even the Trap of this last district, a rock confessedly later than the last secondary stratum that exists, reposes here on the gneiss, as it also does in Mull.

Old red
Sand Stone

Some of these examples may be considered merely as proving the omissions of particular strata; but many of them are, in fact, irregularities of alternation. Lest however any doubt should remain respecting the possibility of these strata being actually transposed, a few decided examples of that nature may be quoted. Hornblende schist is found alternating with every rock in the whole series, as is limestone. The same nearly is true of Argillaceous schist and of Diallage rock. In Jura and the adjoining islands, quartz rock, micaceous schist, and argillaceous schist, occur in an endless succession of alternations; and to these are added, in Isla, Gneiss and Limestone. On the opposite coast of Argyllshire, the alternations of chlorite schist, micaceous schist, quartz rock, and hornblende schist, amount to many thousands; and even gneiss, limestone, and argillacous schist, are sometimes added to these four. The term subordinate, it is true, has been invented to get rid of this objection, as I shall pre-

sently notice more particularly; but it is incumbent on v -28/
the inventors to explain its meaning, and, if it has one, to show that it does not form a subterfuge.

Whatever analogies may subsist between some rocks of the most antient and others of the most recent date, such as between the limestones in both classes, there can be no alternation between these two great divisions, by the very nature of the admission which constitutes this distinction. It remains to see the nature and accuracy of the order in which the secondary strata follow each other.

There are but three distinct and principal rocks in the secondary series, namely, sandstone, shale, and v 2
limestone; although a variety of circumstances, arising from minute changes of character, relative position, or imbedded fossil bodies, give rise, in them, to many different, and often very constant varieties. If these were to be considered merely according to their fundamental distinctions, the result would be, that they are repeated in every possible kind of disorder, and in endless alternations. But to give the subject every advantage, as well as those to which it is really entitled, let all the distinctions that have been made be granted, as far at least as these are really constant, and as far as they are not merely dependent on place; in which latter case, it is plain that the whole question would be resolved into a *petitio principii.*

Proceeding on this principle, we find that a particular sandstone, frequently red, is the lowest stratum in the secondary series. I have already shown, however, that it is wanting between the gneiss and the coal series of Morven, and between the granite and v -27/
that of Sutherland. It does not exist in certain parts of Sky, and it is also absent in Mull, in Airdnamurchan, and in other places which I need not enumerate,

in the same country. Similar deficiencies have been observed in many other countries; so that this is not an indispensable stratum.

Mountain limestone

The limestone which follows it, when present, called in England, the mountain, and the carboniferous limestone, and which seems to have been generally considered as a transition limestone on the continent of Europe, is also occasionally wanting altogether. In Scotland, it is absent, not only in the cases just enumerated where the red sandstone is also deficient, but even sometimes when that is present; so that, in many parts of the same country, the coal strata repose immediately on this oldest secondary rock. If it should prove true, as seems probable, that the upper parts of the Arran sandstone belong to the red marl, then both these, namely the lowest and highest red sandstones, are in contact, and nearly confounded together, in most places. I may pass over the coal strata; as, from their acknowledged partial and independent nature, they do not form part of the present inquiry. It may only be remarked, that such is the irregularity of recurrence among the beds belonging to this series, that in no two examples is that similar.

Magnesian limestone.

Beyond this we arrive at the magnesian limestone of the English series, supposed to correspond to the first flœtz stratum, to the alpine limestone, and to the zechstein of foreign geologists, and followed by the later red sandstone, or red marl, agreeing with their variegated sandstone. A new order of arrangement here begins among the secondary strata, whence we may take a fresh departure. It is not meant to say, that the red marl, much less the associated inferior limestone, is invariably present, even in Europe where it is known to occur; but if there is any series truly entitled to the character of regularity, as well as of universality, using that term in the general sense formerly stated, it is this one. Still, it is proper to remark, that in the red marl series, which is in itself a very complicated one, there is a very irregular recurrence of the different integrant beds.

Ascending from this series, we find, in different countries, various successions of limestones, clays, shales, and sandstones. But our accurate observations on this branch of geology are as yet limited to Europe, nor can we pretend to say that they include the whole, even of this small division of the globe. Hence, the arrangements which have been laid down for the succession of strata from the red marl upwards, may be local, to this extent; while the disagreements of geologists on certain points, may make us fairly doubt whether we do not imagine ourselves possessed of more knowledge, even of Europe, than we have yet acquired. There is still a want of conciliation, for example, between the order of England and that of those portions of the continent which have been best studied; but these are differences which ought to excite no surprise, except in those who have a dormant affection for the theory of universal formations, or who are determined to find the type and model of every thing in the country of their regards. If, as has often here been said, these strata have been deposited in seas more or less distinguished in forms and position, and bounded by original mountains of diverse character, it is a necessary consequence that, with a general analogy, such differences should exist. And if all the seas and climates of such tracts have not contained precisely the same living animals in the same order of succession, it is equally impossible that the fossil remains should agree through all, or be invariably associated with particular rocks and in a particular order.

Limiting ourselves now to Europe and to our present knowledge, there appear to be three leading deposits, in this upper part of the great secondary series, of sufficiently constant character and place, to which all the others can be referred. These are, the red marl, last named, the oolithe limestone, and the chalk. The others are the Magnesian limestone, the Muschelkalkstein, the Quadersandstein, the Lias, the Ferruginous sand, and the Green sand; the two first of which are not thought to exist in England, and of which the ferruginous sand is either not sufficiently distinguished from the last, or else is conspicuous chiefly in our own country.

Formations are Classes of Rocks —

But the details of these deposits, if examined, convey a far different impression from the terms themselves, which are, in a certain sense, conventional associations. Under almost every one of these terms, which, in an exact and mineral sense, are classes rather than single deposits, we find the same substances, sandstone, limestone, shale, clay, and marl, in perpetual alternations, and in such perfect disorder, that, even in immediate connexion, no two parts of the deposit are like each other. Of these beds also, if some are, in one country, considered as subsidiary or subordinate, in others they have received greater notice; as happens with ourselves respecting the Weald clay. Nevertheless we must, for the present, take these divisions as we find them arranged; only cautioning the student against supposing that any one of these terms, Lias, or green sand, means literally what it appears to do, or is a definite substance, or even a definite association of strata.

On the continent therefore, the red marl is followed by a limestone series called the muschelkalkstein; though this is sometimes wanting even there, as it appears entirely to be in England. To this succeeds, in the same countries, a sandstone series, the quadersandstein, sometimes equally deficient, and wanting also in England. That series of limestone and clay or shale, called Lias, is the next; and this seems to be very widely diffused throughout Europe in general, though it is said not to exist to the south of the Alps. I must also here add, that a new deposit of the quadersandstein is said, in some places, to be interposed between the lias and the next class in order, the Oolithe.

This most extensive group of strata is named from the character of some of the limestones which it contains; but the student will commit a great error who considers it as an oolithic limestone, or even as a mere calcareous series. It is an immense group of various strata, of a wide sweep; including, in many places, a great number of limestone beds of very various characters, of which many are far asunder, and separated by conspicuous deposits of other substances. That it is less likely to be absent than some other of the deposits here named, is a natural consequence of an arrangement so inclusive.

Ferruginous Sand

What follows next in order above, is the sandstone called the ferruginous sand in England, succeeded by the more general Green sand, which is also a complex class of substances, though the prevailing part is arenaceous. Lastly comes the Chalk, a limestone of a peculiar character, considerably definite, though including different varieties and substances, and terminating what are considered the secondary strata. For

Chalk

what succeeds, I may refer to the Chapter on the Tertiary Formations.

Of all these groups I must now remark, that although any one may be deficient, there is no instance, as it is said, of the order being inverted; but it must be plain that where an arrangement approaches so much to an artificial order, it would not be very easy to prove an inversion. Assuming therefore certain series or associations, rather than individual substances, the order of recurrence or superposition in the secondary strata, is much more constant than in the primary, or it, at least, appears to be so. But it must be remembered that these may possess certain associations, or series, analogous to those of the secondary, which we have not yet fully discovered, as we appear to have done in the case of gneiss and hornblende schist, but which may possibly come to light when this division of rocks has been more minutely studied and compared in different countries than it has yet been. It is possible, for example, that the intricate series of the quartz rock, and of the micaceous and argillaceous schists of the Western Islands, may be a characteristic and definite series, like that of the Lias or the red marl in the secondary strata, and that it may occur in a similar manner in other parts of Europe. It is easy to see the drift of this supposition; so that I need only add, that it is not a fair comparison between the primary and the secondary strata, when individual beds are selected for comparison out of the one, and whole series out of the other. For the present, it seems prudent to come to no decision on this point, but to wait for further information. It is also evident, that by omitting trap in the consideration of the secondary rocks, and admitting granite among the primary, an appearance of greater irregularity is given to the former than to the latter. In justice, all the unstratified rocks should be put out of the question in this case.

I need only remark further, on the subject of the secondary strata, that it is not easy to see how such a resemblance or identity can, in these cases, be so proved, as to warrant any inference respecting similarity or order of succession among them in remoter countries, or in distant parts of the world. The mineral characters of the various beds of limestone are rarely very strongly marked. The differences among shales and sandstones can scarcely be perceived. It will be hereafter proved (Chap. xx.), that, in distant countries, the identity of two strata cannot be inferred from their organic fossils. Nothing then remains but the juxtaposition, in analogous order, of two or more strata; and this, it is easy to see, is proving the fact in dispute by means of the very thing to be proved.

It was just remarked, that as far as the question of order or disorder, under the limitations thus made, is concerned, the latter appears to arise from omission rather than inversion. In the case of individual strata in a group, whether in the primary or the secondary, or in the coal series, as well as in gneiss and quartz rock, an inversion is as common as an omission; and to what degree that really does extend among the primary, we cannot, for the reasons just given, as yet decide. But in the secondary, it is not yet known for example, that chalk does, and it is not probable, that it will, occur beneath the red marl; though, from the deficiency of the latter and of all the intermediate strata, it might be in contact with the coal series, or even with granite. Still, however, we must not establish this as a canon in the science; because, *a priori*, there appears no chemical or physical reason why it ought to be so. To lay down such laws, is to throw obstructions in the way of our own progress, to fabricate a science instead of deducing one. To do less, by making rules which apply only to the cases whence they are derived, is to do nothing; it is to cheat ourselves with the shadow of a science.

It is not indeed very easy to see what purpose is to be attained by this generalization, if the doctrine of universal formations is abandoned. It is the basis and support of that doctrine, if not the very thing itself. Nor is it probable that, on general principles, there can be a definite order of succession among the greater series over extensive tracts of the globe, any more than among the smaller beds in a limited spot.

It is easy enough to understand how, in one deposit, a constant order, at least as far as relates to the principal strata, should have been preserved. It is equally easy to admit, that confusion may have been produced among the less important, merely by the omission of a few out of a great number of alternating strata of different kinds, without affecting the general integrity and order of the deposit. But till further reasons are produced to show that these deposits are universal, it will be impossible to admit, that wherever rocks occur of similar characters in distant parts of the world, they must necessarily maintain the same order. Further, till they are shown to be of much greater continuous extent than is now probable, it can excite no surprise if, even in Europe, that order of succession should differ. The existence of tertiary deposits, proves that particular collections of strata are limited; and it would be no less unreasonable to expect an absolute correspondence between those of Paris and the Isle of Wight, or among the whole of these that have been discovered, than that the secondary strata should adhere to a constant order all over the world. There is a general analogy throughout nature in the successions, as there is in the characters of rocks; but the particular instances are modified by laws of a local nature, operating in a limited spot, and subject to uncertain modifications in many of these. I might add, that they might even differ in the same deposit or cavity, on a principle formerly laid down, namely, that the present submarine deposits of the English channel, the probable types of past strata, if not the germs of future ones, are not everywhere similar and corresponding in order.

It remains to say a few words on that which has been sometimes used as an expedient for preserving the integrity of this theory. It applies equally to the primary and secondary strata; and the examination of it has therefore been reserved to the last place. The term *subordinate*, already noticed, forms the whole of this contrivance; but it is necessary here to explain its application. If a series be produced in proof of irregularity, consisting of gneiss, argillaceous schist, and quartz rock, and if it is a part of the theory that gneiss only should be found in it, the other rocks are called subordinate. If, in any place, two or more beds of some particular sandstone should alternate with limestone, the integrity, place, and order of the sandstone is preserved for the purpose of the hypothesis, by calling the latter subordinate. In all these cases indeed, it is usual to drop altogether the name and history of the intruding beds. Fidelity

Subordi-
-nate Beds
v. II 47

and logic are here alike made to yield to an imaginary convenience.

If this view of the abuse of a term should be judged too strong, there is no geologist of any reading and experience, who will not confirm the fact of its misapplication to these ends. The evil consequences arising from it are considerable: since it substitutes words for facts, and affords the student a ready mode of husbanding his industry and his reasoning, by stating, not that which is, but that which ought to be. It is not meant to be denied, that the term subordinate may often be properly used; since many rocks do occur in very small portions, in a single and important series of some other. But it is a wide difference to make a legitimate use of a fact, and to pervert it to mischievous purposes.

It will be no small acquisition to the student, if these remarks shall relieve him from that anxiety which must ever follow all attempts to discover what is not, but which he believes to exist; if they shall preserve him from that distrust of his own skill or discernment, which often ends in turning a modest mind from a pursuit in which it finds its observations at variance with those which it is taught to think established.

Of the Succession of the Strata in Britain.

After thus giving some general illustrations of the successions or orders of the strata in confirmation of the views here held out, it will not be considered superfluous to add some examples in detail from the very extensive succession of strata found in this country. In an elementary work produced in Britain, this partiality will be justified to the reader; while, as far as regards the secondary strata at least, no examples equally extensive have been brought forward, in which the order of arrangement and succession has been so satisfactorily ascertained.

In examining this order of succession in the primary class, it is necessary to commence from granite, whereever that is present; and, when it is not, from the lowest visible rock. Whether this method is, in all cases, unexceptionable or not, we must adopt it, for want of other more certain criteria of the proper points of departure. The following examples, out of many more that might have been adduced, will answer all the purposes at present in view.

Shetland offers some of the best examples of series of strata in which the members are, at the same time, numerous, and disposed with a considerable degree of that regularity which was once thought universal and necessary. In that part called the Mainland, the granite is followed by gneiss and hornblende schist, quartz rock, chlorite schist, and argillaceous schist; beds of limestone occurring more than once in the series, both with the gneiss and the micaceous schist. The lowest red sandstone, as the first of the secondary strata, succeeds to the argillaceous schist. In this enumeration, I have, however, given to the order, the advantage derived from the omission of the minuter alternations; or treated the principal rocks as so many distinct series, with minor quantities of others in subordination. The enumeration, by beds, of each, in the manner followed by Ebel, and of which I shall give some examples hereafter, would have presented a very different aspect.

In the same islands, taking Yell, Unst, Fetlar, and the adjoining smaller isles, as one tract, which they may safely be considered, from the intimacy of their association, there is a series still more extensive; and treating it in the same general manner, it may stand thus. The Granite is succeeded by gneiss, hornblende, micaceous, chlorite, and talcose schists, Diallage rock, serpentine, and argillaceous schist. There is no limestone in this series, and the secondary strata are absent. Were this series however minutely detailed, it would be found that there were more than one alternation of every rock in it, and, of some of them, a great number. It may be useful to contrast one of these modes of description with the other; partly for information, and partly to show how arbitrary our method of grouping and omitting must often be, and how little we are entitled to determine what the real order of the series, on the great scale, is, or whether there is such an order at all. By selecting from these beds, at our pleasure, the supposed principals and accessories, almost any hypothetical order might be made to appear the true one. Not to make the enumeration more intricate than is necessary for this illustration, I shall select only two of the least numerous successions from different parts of these islands. Such are

Gneiss
Micaceous Schist
Chlorite Schist
Micaceous Schist
Gneiss
Argillaceous Schist
Serpentine
Diallage Rock
Talcose Schist
Chlorite Schist
Micaceous Schist
Argillaceous Schist

and

Gneiss
Diallage Rock
Chlorite Schist
Argillaceous Schist
Serpentine
Argillaceous Schist
Diallage Rock
Serpentine
Talcose Schist
Micaceous Schist.

Were the reader to add both these lists together, and even to multiply some of the alternations, he would probably find justifiable cause in the examination of the ground itself.

It is extremely rare to find, in Scotland, any series so numerous or so easily reduced to the supposed or hypothetical order. The following, from different parts of the central districts, or from Perth, Inverness, and Aberdeenshires, are much more common. Granite here is followed in this manner, taking first the most numerous series, and, in a similar way, omitting the minor alternations.

Gneiss
Limestone
Quartz Rock
Gneiss
Micaceous Schist
Argillaceous Schist,

and

Limestone
Quartz Rock
Hornblende Schist
Gneiss
Micaceous Schist
Gneiss
Argillaceous Schist

the lowest red sandstone of the secondary class succeeding.

Detailing more minutely two examples from the same places, they afford the following order, and in such a manner that it seems impossible to associate the different strata in groups or subordinations, at least in many cases.

Gneiss
Limestone
Hornblende Schist
Quartz Rock
Limestone
Gneiss
Limestone
Micaceous Schist
Chlorite Schist
Hornblende Schist
Limestone
Micaceous Schist

Limestone
Gneiss
Hornblende Schist
Micaceous Schist
Gneiss } more than once
Quartz Rock }
Limestone
Micaceous Schist
Gneiss
Micaceous Schist } more than once
Chlorite Schist }
Argillaceous Schist

Gneiss
Micaceous Schist
Argillaceous Schist
Chlorite Schist
Argillaceous Schist

Limestone
Chlorite Schist
Argillaceous Schist.

It being unnecessary to quote more of these, I shall give some local examples of an order which may be called inverted; supposing the direct one to be the succession of gneiss, micaceous schist, quartz rock, limestone, and argillaceous schist, as formerly presumed, and omitting the other members of this class.

Granite is succeeded, first by argillaceous schist and then by gneiss, in Iona and in Bamffshire; and, in Rossshire and Sutherland, after the gneiss, there follows another argillaceous schist. In Sutherland, in another place, after granite, there come in order, gneiss, quartz rock, bituminous limestone, quartz rock, and gneiss. Numerous other instances of the same nature might be adduced, but it would be superfluous.

A few examples of short series, will tend, even more than the former irregular ones, to show the want of a fixed order among the primary strata; and that they were alluded to in the first part of this chapter, needs not prevent a tabular view of them here, as it will render these illustrations more useful. Their leading character consists in the omission of most of the primary strata, and, in some instances, of some of the secondary ones also; so that they might equally have been enumerated in the next division of this subject, where it will indeed be necessary to refer to them. Most of the primary strata are wanting in the examples which immediately follow.

Granite	Granite	Granite
Gneiss	Micaceous Schist	Argillaceous Schist
Secondary Strata	Secondary Strata	Secondary Strata
In Caithness and Sutherland.	In Arran.	In Arran, Cornwall, Aberdeenshire &c.

In the following, all the primary strata are wanting, so that the granite comes into contact with the secondary.

Granite	Granite
Old red Sandstone	Coal series, or Oolithe and Lias with Lignites
In Aberdeenshire and Caithness	In Sutherland, and the Western Islands &c.

v. 271-273
II 318. –

I shall terminate these illustrations by naming some examples where the following strata occur in a succession of alternations so extremely numerous, that it is fruitless to draw out a list of them, as I should scarcely know where to cease.

In the quartzose isles of the West of Scotland, or the chain of Jura and Isla, these alternating substances are.

Micaceous Schist
Fine Argillaceous Schist, or clay Slate
Micaceous ditto, or fine Graywacke
Arenaceous ditto, or coarse Graywacke
Conglomerate ditto, or coarsest Graywacke
Quartz Rock.

In the series which terminates the Highland mountains towards the low country, and in a similar series in Argyllshire within the micaceous schist, the following beds alternate in a similarly repeated manner.

Fine Argillaceous Schist, or Clay Slate
Coarse ditto, or Graywacke
Chlorite Schist
Micaceous Schist
Gneiss
Limestone

In another series in Argyllshire, the alternating substances are.

Hornblende Schist
Actinolite Schist
Quartz Rock

Micaceous Schist
Chlorite Schist
Limestone
Argillaceous Schist
Gneiss.

In this particular case, the alternations amount to some thousands.

To proceed now to the secondary strata, it will be best to give the order of succession in England, and in an entire state, as it is conceived to be determined by the numerous geologists who have bestowed great labour and care on this department. The groupings of the beds, or the divisions into series, must stand on the authority or opinions of observers whose ability and industry are admitted, and who are too well known to need mention here.

On the Succession of Strata in England and Scotland.

The lowest bed of the secondary strata of England, is the old red sandstone, being the first of the secondary rocks in the artificial classification. It must not however be considered as a simple rock; since, besides the conglomerate which is essential to it, it sometimes contains shales and limestone, and, occasionally, coal.

The next bed in the order upwards, is a limestone containing a few fossil remains, and known by the name of mountain and of carboniferous limestones. In this rock also, there are found smaller beds of shale, and, occasionally, of sandstone; much more rarely, of coal. A sandstone called the Millstone grit follows in some places; but it seems to be one of the most limited of these leading strata, while it does not appear very satisfactorily proved that it does not form a part of the coal series.

Millstone
grit.

27

This complicated series, the next in order when present, is often of an enormous depth, consisting of sandstone, shale, clay, and limestone, with intermediate beds of coal and with vegetable remains. This collection of strata appears less consistent as well as less constant than most of the principal series; being distributed in distinct situations which, from the discrepancy in the recurrence of the several beds, appear to have been in a great measure independent of each other.

Thus far, the general dips or positions of the beds are marked by the same kinds of irregularity which attend the primary strata. They are commonly much inclined, and sometimes reversed and dislocated; although, taking a considerable tract, there may be a prevailing dip as well as a prevailing direction. The coal series is particularly noted for its irregularities of this nature. Beyond this point, however, a new arrangement appears to commence; and the strata which follow, maintain a parallel order to each other, with certain local exceptions that will be noticed in their proper places; while they are, at the same time, much more regular, and less subject to high angular positions. This new order is analogous to that which occurs between the primary and the secondary classes, while it is generally more strongly marked.

Where the series is most complete, the coal strata are followed by the magnesian limestone, as it is called, or by a conglomerate formed of fragments of the lower calcareous rock and others, cemented by that substance. To this succeeds the very important stratum to which, in England, is given the name of new red sandstone or red marl.

The red marl is not however a simple substance; but is in itself a complicated series, consisting of conglomerate rocks, fine red sandstone, shales, clays, and marls, and especially remarkable for including gypsum and rock salt.

The magnesian limestone and the red marl are associated by some English Geologists, under the name of a *formation;* and in a similar way, all the strata, of whatever nature, consisting of various limestones, sandstones, shales, and clays, which lie between the last described bed and the ferruginous sand, are united in a similar group, by the name of the oolithe formation. I must refer to the authors themselves, and particularly to Conybeare, for all points of a local and minute nature; and shall here merely give the principal beds as they are enumerated, where the series is most complete.

I ought still however to remark, that not only is this principal *formation* divided into three, viz., a lower, a middle, and an upper one, but that there are even inferior subdivisions, since the lias, for example, is itself a series and not a single rock.

Lias

The first which thus follows is, in England, known by the name of Lias, and it consists of limestone of various characters, in numerous beds, alternating with clays and shales. It contains a great variety of organic remains, chiefly marine, with some very remarkable amphibious animals, of extinct genera and species.

It is here but justice to the general reader to state, that all those geologists who have investigated the English strata, have not agreed in their manner of grouping the inferior beds. At this particular point, for example, certain marls, clays, and sands, have, by some, been thrown into a group distinct from the beds, and inferior to the next rock, or the oolithe; while, by others, the clays are enumerated as belonging to the lias. On the respective values of opinions of this nature, it would be needless to attempt a decision, were it possible.

Oolite

The remaining strata then, proceeding upwards, in the lower division of what is called the oolithe formation, are a sand of various characters, a coarse, somewhat oolithic limestone, a stratum of clay sometimes including fullers' earth, and a limestone called the Gray oolithe. The middle division contains, in the same order, a calcareous and a siliceous slaty rock intermixed with sand, shale, and some coal, three limestones, supposed distinct, and called forest marble, cornbrash limestone, and Kelloway rock, and a clay called Oxford clay, or fen clay. In the last, or uppermost, division, are comprised a calcareous sandstone, a limestone called Coral rag, another limestone, called the upper oolithe, a clay called Kimmeridge clay, a third limestone called Portland stone, and, lastly, the Purbeck inferior group; consisting of limestone, shale, and marl.

It is by no means clear that the arrangement of these is so regular and constant, and the different beds of so much importance, as to require the decisive names which have thus been conferred on them. There also appears much that is purely arbitrary in those arrangements into the minor series whence these characteristic terms are derived; since, in one of these artificial groups, the same substances exist as are found in another part of the principal deposit, forming or entering into another series. Some of these deposits, indeed, seem to be as irregular in their disposition as the strata of the coal series, which, in their general characters they so much resemble; nor does it seem as yet possible to reduce them into a system in which all observers shall agree; while the attempt to refer particular beds in distant places, to some favourite deposit found elsewhere, is a no less frequent source of difficulty than of fallacious generalization.

The next series of the English Geologists is called the green sand *formation*, comprising three inferior groups. The first of these is known by the name of the ferruginous sand; very constant in its character and position, and forming a remarkable member of the whole series of the English secondary strata. It contains masses of sandstone and beds of ochre, clay, and of fullers' earth. Above this is a complicated bed of clays, called the Tetsworth clay, and that again is followed by another arenaceous deposit, called the green sand, including some of the most recent sandstones, together with limestone.

The next and highly important series is the Chalk, divided into three beds; the lowest known by the name of chalk marl, the next being pure, and the third containing flints. Some Geologists however arrange the chalk marl in the last group, and subdivide the chalk beds more minutely. This member is sufficiently remarkable, both in its mineral characters and the nature of its contents, to be recognized and compared with other similar deposits in distant places; and it accordingly offers one of the most striking proofs of the very considerable extent occupied by some of the secondary strata, though it must nevertheless, like the greater part of these, be considered a partial deposit, when compared to the remainder of the Earth's surface. It is remarkable that this stratum has, in some places, undergone violent dislocation and changes of position; many of the beds on the southern coast of England being even in a vertical position, while the fractured state of their contents shows also, that these changes have been posterior to the time of their consolidation. This case offers an excellent example of partial revolutions; and serves to prove how imperfect a criterion for distinguishing the artificial classification of primary and secondary strata, is derived from such circumstances alone.

The Chalk, which is the last of the strata esteemed secondary, is followed by various substances which have been grouped in different manners by different Geologists, but of which the localities are partial and limited. The plastic clay occurs first, and consists, not only of various clays, but of sand and gravel of various qualities. The London clay, which succeeds, comprises also calcareous sandstone; besides which it contains many organic remains, terrestrial and marine. The strata which succeed, consist chiefly of marls and clays; but as the details of objects so local are too minute for the present purpose, I may refer to Mr. Webster's accurate account; particularly as the importance of the tertiary or partial deposits, is such as to demand a separate consideration.

It must now be observed that the series of secondary strata in England, thus given in a complete form, as it is thought, by those who have investigated this subject most minutely, is far from exhibiting this succession in any one place. It is not merely that the whole series terminates at some point beneath the uppermost or London clay, as for example, at the coal series, or the red marl, or the lias, but numerous members are in many places wanting. This succession must therefore be considered as, in some sense, an artificial one; constructed according to some presumed principles in the science, and a picture of what Nature might have given, rather than of what she has actually produced. As it would be impossible in a work of this nature to enter into the details of this part of the subject, I must needs refer to the valuable work of Messrs. Conybeare and Phillips, where all the information that is required will be found.

But as that work does not contain any view of the Scottish strata, I may here give a sketch of some examples of successions among the secondary ones in that country; particularly as it exhibits some deficiencies, which, besides being remarkable in themselves, are rendered more interesting by the intimate geographical connexion of these two parts of one island. The reader will not, of course, expect here an account of the geology of Scotland, nor any details, either very minute or numerous. I may even add that no greater accuracy of description is adopted, than was necessary for illustrating the particular subject under consideration.

In that country, the deposits of secondary strata may conveniently, if not very geographically or correctly, be divided into four. Of these, the northern includes Caithness with parts of Inverness and Moray, while the middle forms a very extensive tract to the south of the Highland primary district. The Western is much disjointed and scattered on the Western Highland shore and among the islands; and the southern is more or less intimately connected with the corresponding strata in England.

The first of these is nearly limited to one bed, namely, the lowest sandstone; but one conspicuous tract of mountain limestone occurs, together with smaller fragments, and, in one place, it is immediately followed by a series of coal strata or lignites, already noticed, as found in Sutherland, and which also immediately rests on the granite.

A great part of the middle district, also, consists solely of the lowest sandstone; and the remainder presents a series of coal strata succeding to that. As, in some places, a limestone is interposed between these strata and the lowest sandstone, analogous to the lowest or mountain limestone of the English, it may be fairly considered that there is a perfect analogy between this deposit and that of England as far as it extends. But that extent is limited; since, in many places, this limestone is wanting. Hitherto, with some rare exceptions, the coal strata have been found to reach the surface everywhere in this tract; neither the magnesian limestone, which follows in England, nor the red marl stratum, forming a part of the series. Nor are any of the strata that lie above those found in it, whatever slender indications there may be; so that the chief Scottish secondary series, even where most complete, terminates with the coal deposit.

The Western secondary deposit is rendered very obscure by its scattered and disturbed position, and by the very limited quantity of it which exists. In some places, as at Campbelltown, it exhibits the lowest sandstone, while in others, as in Morven, that is wanting; the lias, with the coal, or lignite, reposing immediately on gneiss. In some also, there is an inferior limestone; but, however connected with the lias, it appears, in its relation and in its characters and fossils, to correspond to the mountain limestone of England. The actual coal series where it exists, is very limited, and must be considered to belong to the first coal which follows the great coal deposit, or to the lowest of the lignite deposits; in which case it will correspond to that of Whitby. This deposit is neither accompanied nor followed by any other strata exactly analogous to the English; with the exception, in some parts, of a sandstone which must be classed with the green sand of the southern division of our island.

It is easy enough unquestionably, with the possession of limestone, sandstone, clay, and shale, since there are virtually no other strata, to make or assign any order which a geologist who is anxious only for his theory may wish. If any member of the great secondary series may be wanting, as is the fact, and if, out of these four rocks, the characters and aspect of any one, or of the whole, are so indefinite and vacillating as we know them to be, while the nature of the organic remains are further incapable, separately, of proving either an identity or a dissimilarity, as is also true, it is evident that any assertion may be made on such a subject, without an effectual risk of contradiction. On such an hypothetical system, it is perfectly easy to reduce the Scottish series, or any other, to an absolute conformity with the English. They may be the same; but it is better to be cautious in deciding, lest we substitute an artificial fabric for real knowledge. Those who blamed the school of Freyberg for reducing the whole world to the model of Saxony, should be cautious lest, in another department of their pursuit, they fall into the same error.

The southern deposit differs in no respect from that of the central district. The red sandstone is followed, partially, by the mountain limestone, as it is termed; and the whole terminates with the strata that belong to the coal series, which must be considered as a mere continuation of the English one in the conterminous district.

It is nearly useless to inquire whether the superior strata of England have never been deposited above the coal series of Scotland which most resemble those of the former country, or whether they have once existed and been worn away. This is not, at any rate, the place for such an inquiry. If it is true of the middle deposit, as, to a certain extent, is not improbable, it may equally be so with regard to the northern and western; though, in this last case, the proper coal series is wanting, and not only this, but very often, the rocks beneath it; while the causes of waste, supposing this to have been the fact, have operated to a different effect in the two cases.

CHAP. XV.

On the elevated submarine Alluvia.

The subject of the present chapter is so intimately connected with that of the following one, that had it not been for the novelty of these views, and the unwillingness of geologists to receive an arrangement of what they have so long misunderstood, I should have united it to that one, and thus given a general theory of all the deposits of this nature which are later than the chalk, and which have been so confounded under the term tertiary. But I have another reason for thus preserving it distinct. It was thus printed long ago in the *Quarterly Journal;* having been separated from the latter on account of the length of the whole; thus enabling others to profit by those views, in claiming as a recent discovery, what was also written many years before it was printed, including the theory of the most difficult of the tertiary strata, as well as of the latest revolutions of the earth.

As it now stands, it therefore proposes to distinguish this particular case of strata, or deposits, from those which are found in basins, be they marine or lacustral, or both united; as it furnishes the special evidence for the following views of the most difficult of these: showing, namely, that some of the basin-shaped deposits have been elevated to their present positions by analogous causes. And, as portions of the bottom of the present ocean, they require to be separated, if we are really desirous that Geology shall not continue to be a disgraceful chaos. It is by considering causes, not facts alone, that this science has already become what it is, in distinguishing the primary from the secondary strata, the stratified from the unstratified rocks, the great coal deposit from the Lignites, and far more.

Hitherto, these deposits have, as yet, been certainly found only in Italy, but they are probably not limited to that country, if the present theory be correct. For the bare facts themselves, we are indebted to Brocchi; but as he has singularly failed in his attempt to explain them, I have endeavoured to supply that deficiency; without, however, presuming to suggest any alterations in his views of the facts themselves. Where, in some cases, those seem deficient, I have merely proposed amendments on his own principles. It is an extreme abuse, on the part of systematic writers, to determine what an observer ought to have seen; as this practice may be made subservient to any hypothesis, and as it renders all observation useless: but there is no rule of philosophy against the attempt to reconcile the observations of others to general principles, where the observers themselves may have failed. The Italian alluvia in question have been hitherto classed with the tertiary or fresh water deposits, without any attempt at distinction, or at an explanation of their origin: while these Subapennine formations, as they were called, have been held to contain great mysteries, which were hopeless, but likely to furnish the clue to the later revolutions of the Earth. That mystery is, I trust, here solved, by a very simple review and arrangement of plain facts.

The task of Signor Brocchi would not have been left to another, had he paid more respect to the theory of his countryman Lazzaro Moro, to whom this science owes a debt which his successors have been most unaccountably unwilling to acknowledge. That a late illustrator of this theory under a much more modern name, with the advantage of a personal examination, did not form the same conclusion, valuable as it must have been to the System which he defended, might be used by his admirers as an argument against the view here given; but the conclusions of one philosopher form no rule for those of another; and personal examination has not always discovered Truth.

For the entire geography of these appearances I must refer to Brocchi's own writings: it is sufficient here to say, that this deposit occupies many low situations, and also forms or covers a range of hills at the foot of the Apennine; occurring in various places, as in Piedmont, near Placentia and Parma, and along the north side of this ridge to Otranto; while, on the south, it is found at Orvieto, Rome, Terracina, and elsewhere, thus skirting the ridge on this side also. In the same manner, it is found at Vicenza and Verona, or at the foot of the Alps as well as the Apennines; so that the term Subapennine is not very well chosen. By putting together Signor Brocchi's facts, as he has himself forgotten to do, it is indeed easy to see that nearly the whole promontory of Italy is more or less covered by this interesting deposit, that it does not necessarily form hills, and that it is deficient, only where its deficiencies may be accounted for, either by the waste and absence of the superficial parts on the higher ridges of the fundamental mountains, or by volcanic eruptions and earthquakes, or lastly, by the action of rivers, which have washed it away, or have covered it with other alluvia of the usual recent terrestrial origin.

The general deposit, given by Brocchi under a common term, consists of two beds, and it is essential to distinguish these where they are regular; because, being confused in some places, they have sometimes been described in a careless manner, as if this was a part of their natural character. Thus they have been said to consist of marl, sand, and gravel, together with sandstone and occasional breccias, containing various marine and terrestrial remains. In a general sense, the beds may be considered horizontal; or rather, as placed at low angles; being, therefore, unconformable, under the usual variations, to the inclined calcareous strata of the Apennine on which they lie.

The marl bed, which is the lowest, is, in some places, of an argillaceous nature, in others, argillo-calcareous; often also containing mica. As it is sometimes wanting, the upper bed, which consists principally of sand and gravel, occasionally rests immediately on the solid and fundamental limestone. The lowest stratum is the repository of different mineral substances, such as the sulphates of lime, strontian, and barytes, and of flint, quartz crystal, pyrites, bog iron ore, sulphur, and bitumen. Salt springs also rise out of it, and it occasionally gives vent to hot water and sulphuretted hydrogen; from the vicinity, probably, of volcanic materials. The upper bed consists of siliceo-calcareous or siliceous sand and gravel, often containing mica and yellow ochre; while in some places, as at San Marino and Volterra, it becomes a solid sandstone. It does not everywhere cover the marl bed, being occasionally deficient. This deposit, it may be added, is sometimes accompanied by the partial breccias just noticed, consisting of fragments of the older rocks, occasionally containing shells.

If we take both these beds together, as Brocchi has sometimes done from not seeing the value of the distinction, the organic remains contained in them exhibit great confusion of origin. They comprise numerous marine objects, consisting of shells and fishes; but these are far more abundant in the marl than in the sand; while very extensive tracts of alluvia are found without any. The shells are said to be sometimes similar in both beds; but it is important to remark, that, where they abound, they are found associated in families; a proof that they have not been transported, but that they now lie where they were originally produced. Some of these animals are admitted to exist in the present seas of Italy, while others are supposed to be exotic or else unknown; but a great deal of obscurity has been introduced into this latter part of the subject by Volta and others; partly from ignorance of this branch of natural history, and partly from the theory to which they thought it necessary to make every thing conform. It is essential to observe, that the shells often retain the ligaments, and the fishes their animal matter; a proof of the suddenness by which they have been elevated above the waters and dried. Nor is it less important, that, among these, are found a far greater number of species analogous to and identical with existing ones, than even in the other strata which follow the chalk. Out of 240, there are 139, according to Brocchi, of this description; a fact which assists in indicating a date for these deposits, probably more recent than those of some other tertiary formations, and depending on a separate and local cause.

Besides these more common marine remains, there are found the bones of whales and dolphins; and even entire skeletons of this nature have been discovered at elevations of 1200 feet above the sea. It is further remarkable, that the bones of the whales have been found incrusted with oyster shells, and that they are almost always in a state of high preservation; a proof that they have not been brought from a distance, or that these are not transported alluvia. The terrestrial remains generally occur a few feet beneath the surface, and are therefore commonly in the sand or gravel, or in the upper bed; but as that bed is occasionally absent, they are also found in the marl. They consist of the bones of the Rhinoceros, Elephant, Hippopotamus, Mastodon, Urus, and Elk, together with the horns of Stags; and to these must be added vegetable remains, consisting of trunks and fragments of trees, together with leaves often little altered, fresh water shells, and lastly, fragments of travertino, or alluvial rocks, with vegetable calcareous incrustations resembling those which are daily formed in situations where solutions of carbonat of lime flow.

Besides these two remarkable beds, many parts of Italy present superficial strata, some of which are peculiar to itself, while one is common to all countries. This last is the ordinary alluvium of rivers; such as that of the Po and Adige to the northward of the Apennine, and that of the Tiber to the southward. Those which are peculiar to it, are the solid calcareous alluvial rock called Travertino, loose tufaceous matters of the same nature, and volcanic tufas. The plain of Sarteano, the Maremma of Tuscany, the Solfatara, and the vicinity of Rome, offer examples of these strata. The calcareous substances sometimes contain fresh water shells and vegetables; nor are these always absent, even from the volcanic tufas.

Hence arises a confusion which requires to be explained, because it has very much obscured this subject. And the chief source of this consists in the transportation of the volcanic substances, and in their cementation by means of the calcareous waters which flow from the Apennine. In consequence of this, they sometimes contain matters, the presence of which would otherwise be unaccountable, such as vegetables, and land or river shells. In the same way, they alternate, or are strangely and irregularly intermixed with the travertino and the loose alluvia of the rivers; while they are also found in places far from the vicinity of recent volcanoes, or from even the suspicion of antient ones.

It is easy to comprehend the fallacies that must have arisen from misapprehending the real nature of these appearances. When also an opinion of their unintelligible derangement had once been adopted, much more confusion than was actually present was supposed to exist, though a little attention would have solved all the imagined difficulties. Had Brocchi originally proceeded on a proper theory, it is probable that he would have found every thing easy, and have rendered it equally so to his readers.

Though it is said that similar shells are sometimes found in both the alluvial beds, and that the more conspicuous marine remains occur in both, it is decidedly stated that these are far more numerous in the marl bed than in the arenaceous one. As I have undertaken to prove that the lower or marl bed, at least, is a marine alluvium, and the upper probably a terrestrial one, it is necessary to try to reconcile these anomalies, as well as that which consists in the confusion among the volcanic tufas and the alluvial substances. The entire absence of all organic remains requires no explanation.

Where the terrestrial alluvia are wanting the organic substances that would otherwise be found in them, must necessarily appear to lie in the marine or lower stratum, however slightly covered or truly superficial they may be. If even found somewhat deeper, it is not difficult to understand how this might happen, as well as how the marine remains may occasionally occur in the upper alluvia. Revolutions of the surface, and principally from partial transportation by rivers, must inevitably have generated much confusion of this kind, capable, even in the hands of a good observer, of misleading him in his conclusions, unless previously on his guard to distinguish appearances which, even then, are often very difficult to discriminate. Occasional marks of transportation might easily be overlooked over an enormous space, when the principal facts were of a different nature; as these latter would form a sort of standard for the whole, and would naturally lead to a neglect of such petty variations as seemed to be uninteresting.

Not to prolong this examination too far, I shall merely suggest two more circumstances which may easily prove sources of error in reasoning about these Italian alluvia. It is far from certain that the two beds can every where be distinguished merely by their natures, exclusively of the remains which they contain. A sandy stratum must necessarily in some places have formed the bottom of the sea, as well as a muddy or marly one. Thus the marine alluvium may easily be confounded with the terrestrial one; beds of alluvial matter not admitting of that separation which so generally marks different solid strata, even where the nature of the two beds in contact is the same. It is possible indeed that much of the apparently terrestrial alluvium is itself marine; a suspicion which, it will be seen in a future chapter, attaches to many alluvial deposits. It is also notorious that volcanic eruptions and earthquakes have produced great confusion, even in recent times, in many parts of Italy: and when we consider the great number of antient volcanoes in that country, we need be at no loss in assigning reasons for disturbances and anomalies in the appearances of the superficial strata.

The proposed explanation of these appearances is perhaps already obvious to the reader. It is that Italy in general, is covered by one marine stratum in which the organic remains lie in an alluvial bed, untransported and undisturbed; and that, above this, there lies a terrestrial stratum, however originating, and analogous to those of other countries, which contains the remains of land animals, similarly analogous, in all respects, to those which are found in most other parts of Europe.

It remains to explain this state of things, or to give a theory of the alluvial deposits of Italy. That theory, if just, ought to be applicable to similar cases of marine alluvia found high above the level of the sea, should such hereafter be discovered in other places; and it will thus furnish us with a new key for the solution of a certain set of geological phenomena, for which no other branch of any of the general theories provides an adequate explanation. It is important to remark how accurately this partial theory ramifies from the general one here adopted respecting the present positions of strata; and how valuable a test of any theory it is, to be thus provided with the means of explaining appearances that could not have been anticipated when it was formed. Had Lazzaro Moro taken a wider and more accurate view of the circumstances by which he was surrounded, the present explanation would not have been required.

In the phenomena that have been described in the eighth Chapter, the positions of solid rocks alone, containing marine remains, were examined, and the causes assigned, as far as the appearances permitted.

In the present case, we see the germs of these very submarine strata, exposed before their consolidation, and probably presenting the appearances which they do, merely because they are of more recent date. And instead of being compelled to seek for causes by a circuitous and analogical road, we find these at hand in the general volcanic nature of the country under review; while, in some places, we can almost trace the very cause itself in action.

In different places, and in Italy very particularly, it has been observed that the relative level of the sea and land is subject to change, and that it has, in past times, undergone frequent alterations. The present case may be considered an extreme one of that nature; in consequence of which the bottom of the sea, together with its consolidated alluvia, has been raised above the surface of the water, so as to have become dry land. Thus it is easy to account for the presence of marine remains, as well as for their existence in that singularly undisturbed state which has been described.

It is equally easy to account for the proximity of the marine and the terrestrial remains, as also for that of the alluvia which respectively enclose each. Whatever cause or causes generated the usual terrestrial alluvia that occur all over the world, these are thought to have been deposited, in most cases, upon naked rock. In this particular one, they have settled on a previous alluvium of a different character, and, as far as our present imperfect observations go, solitary. The apparent interference of the two classes of organic remains, follows of course. If that interference is ever greater, so as to amount to a real mixture or alternation, it is explained by a variety of circumstances, consisting in more recent changes from the actions of rivers, and from volcanic deposits; and in the imperfection of observations, the real bearings and value of which were not anticipated by the observer.

It follows that the elevation of the land of Italy which is the origin of these phenomena, is to be attributed to the same causes that are now, there producing smaller changes in the relative level of the sea and land, by elevating the latter. They are the same volcanic actions which raised Santorini from beneath the ocean, and which have produced the phenomena of the Coral islands, detailed in a following Chapter. Of whatever date these events may be, they are anterior to all history.

It is now evident that if a similar occurrence were to take place at present, the submarine alluvial stratum with all its imbedded remains, would exhibit the same appearances as the lowest of the Italian beds does: and, that the skeletons of whales should be found in an entire state, at elevations of 1200 feet above the level of the sea, is no more surprising than that they should be found at all. This particular fact is, however, important, as showing the vertical extent of this elevation, just as the geography of the marine remains demonstrates that of its superficial one. For want of more accurate information, we may here take these as Signor Brocchi has given them, for the extreme limits both ways; and thus we can estimate what Italy was before this change, and how much of it has been the consequence of a volcanic elevation, more recent than those extensive changes of the same nature which caused and determined the present general distribution of the land.

The general height of the Apennine is well known; and, on the present supposition, the whole of that chain, from its greatest elevation down to that of 1200, must be supposed to have formed a ridge rising above the sea; taking the skeletons, from Brocchi's facts, as the extreme measure. I need not extend these conjectures to the side of the Alps, as the reader can easily pursue these speculations at his leisure. It is probable that at the period at which modern Italy was produced, the whole of the central chain experienced a fresh elevation to the altitude of at least 1200 feet, and over a superficial space which reaches from Otranto at one end of the country, to Piedmont, and to the foot of the Alps at least, generally, on the other side. If others choose to imagine that only those parts were thus elevated which now possess the submarine alluvium, this would make no difference in the general views, since that force which was sufficient to move so large a part of Italy, might as easily have moved the whole. This is a circumstance that might, however, be put to the proof, by examining the stratification of the Apennines in a proper manner. Some dislocation or discontinuity in the order of the stratification will be found at a certain elevation, if this supposition be correct; and I may here point out to those geologists who may have an opportunity, the interesting circumstances of various kinds which still await them in Italy, from the views of the nature of that country which I have here given. If it should be suggested that the whole of Italy, even to the highest point of the Apennines, was raised at one period from beneath that ocean in which the limestone of this ridge was formed, the absence of the marine alluvium from the higher parts, would be accounted for by denudation.

Though these phenomena may not possess so high an interest as the great elevations of the continents, they are of a much more impressive character, from the greater facility with which we connect the causes and the effects. The others we view through the mist of ages so distant, that they excite in us no personal interest, while we often feel inclined to doubt conclusions attended by consequences revolting to our narrow experience. It is necessary yet to point out one collateral circumstance, which is not only interesting in itself, but which strongly confirms the views here held out. That is, the suddenness or rapidity of the action which produced these important events. This might be concluded from the undisturbed state of some of the shells and skeletons already mentioned; but it is still more strongly proved by the preservation of the animal matter in the ligaments of the bivalves, and by the condition of the fishes of Monte Bolca, belonging to the lower, or marine alluvium, in which the muscular substance is converted into a kind of glue: a fact observed in no other case. The well-known specimen, in which one fish is thought to have been arrested in the act of swallowing another, proves the same thing. I may here also observe, that the condition of the fossil fishes of Iceland throws light on this remarkable deposit. These are found imbedded in an indurated mud or marl, at Patriks Fiord, where it is said they are now in the act of being formed. The fish, in a living state, or perhaps but just dead, seems to have been first entangled in a soft mud, that has afterwards been firmly attached to it by means of the animal matter which has mixed itself with that substance; while the harder parts, or the bones and the scales, remain unchanged. Thus the nodule which encloses them is first produced, and it remains imbedded in the surrounding materials.

In quitting this subject, I must point out to geologists, the propriety of examining all the countries which are analogous to Italy; since the same circumstances respecting the alluvia may possibly exist in many other places. It is scarcely necessary to name those where such phenomena may be sought for; though, as being the most easy of access, and as presenting the most satisfactory examples of volcanic elevation, I may point out Sicily, and Auvergne, together with the Azores and the other volcanic islands of the African coast, as well as St Helena, Ascension, and perhaps, Owhyhee. It ought also to be entered among the perpetual subjects of retrospect in every geologist's recollection, that as all the supramarine land has apparently been elevated, by some causes, from the bottom of the sea, there may be submarine alluvia beneath terrestrial ones in many countries which show no traces of a volcanic nature, or of a volcanic origin. It is quite possible that this may have been the true source of many of the appearances connected with alluvia, and with fossil remains of different origins, that have been the causes of so much trouble to observers. I shall have occasion hereafter (in the twenty-first Chapter) to show that the elevations of the land have probably taken place at very distant periods, and that the causes operated through a long series of ages. Hence there may be a chain of intervals in time, connecting the most remote catastrophes of this nature with that of Italy, and uniting even this one, with the elevation of the Coral islands, described in the seventeenth Chapter, and with the still later formation of volcanic islands. Among some of these, we might therefore expect to find appearances analogous to those which form the subject of this discussion; as we can scarcely conceive such an extensive elevation of rocks, without an accompanying one of the unconsolidated materials also, which chanced to be present. It is sufficient however, to have suggested this possible case.

I remarked, at the commencement of this chapter, that the deposits here discussed had been confounded with the other recent or tertiary strata, Unquestionably, there is a similarity in the strata and in the imbedded substances, even where it is demonstrable that the former are the produce of æstuaries, or of basins; but it is one that is plainly necessary, under whatever difference of circumstances they may respectively have been produced. In both cases, they could have been but the materials which are deposited under sea water, and, in both, they must have contained analogous animal remains. But it must be evident, that, while those remains may present peculiarities in the one situation, not existing in the other, the alluvia under review occupy positions very different from those of any æstuary or lake that can be imagined. It will yet however remain to be seen, whether some, even of those, may not belong to this division; as I shall soon proceed to show that analogous causes are required for the explanation of many of those which occupy elevated positions.

CHAP. XVI.

On the Deposits called tertiary and fresh-water Formations.

GEOLOGY yet wants a fit name for those deposits of strata which are later than the Chalk. If the older rocks, up to this point, could be rightly distinguished into primary and secondary only, the word tertiary would be sufficiently appropriate; but under the better classification which I shall hereafter propose, some other distinction will be necessary. The term fresh-water formations is in every way objectionable; since it confounds the deposits of antient marine æstuaries or basins, with those of fresh-water lakes.

On this subject, our information is still imperfect, rapidly as it has increased of late; partly because of limited observations, and partly because of hypothetical and false views respecting some of the principal deposits; misleading other observers to substitute imagination for facts. But I hope to show that many distinct things have been confounded under one term; and with the consequence, as I trust, of hereafter procuring better observations from the geologists who may undertake this subject. When this shall happen, there will be little difficulty in filling up the details of what I must here give in the most general manner.

Whatever confusion may have been made on this subject, this general character belongs to all; to those which properly demand the present place, to the strata mentioned in the last chapter, and to the alluvial deposits of the æstuaries of the present ocean; namely, that they are found lying on the chalk, or on whatever stratum of the great marine series happens to be uppermost, and under a want of relation or parallelism to those strata. Thus they are easy to discover and assign; and it is no credit to geologists that they were so long overlooked, and so perpetually confounded with the secondary, and even with the primary strata, as had been done in the case of the "bituminous marl slate." I shall commence by making those distinctions among them which have not yet been made; and thence, as I hope, elucidate the whole subject.

I must first reject entirely those strata, be they hard or loose, and whatever remains they may contain, marine, or terrestrial, which occur in any place or country, where it can be shown by geographical investigation, that they have formed a portion of the bottom of the actual ocean. The remark, thus stated, seems so simple, that every one will accede to this exclusion: and yet that distinction has been so little made, that many of the tertiary strata described, are of this nature: as these discreditable errors have produced a very great part of the confusion which has encumbered this subject. And let it be remembered, that if the bottom of the present ocean extends to the foot of the mountains of Upper India, as to those of British America, covered by terrestrial alluvia, there are thousands of similar inland places in the world, where the same facts must exist; as it is most certain that these deposits have been often confounded with the proper tertiary strata. And these are the cases also, where the alternations of marine and terrestrial remains especially occur; under circumstances precisely the same as are now taking place on many sea shores, and especially at the æstuaries of rivers. To class these with other tertiary strata, is to place an effectual bar to all knowledge or hope of order: they must be ranked with alluvial formations; for to those they as properly belong as the present sands of our sea shores. In all else, it is a mere question of time.

The last chapter has also excluded a whole division of deposits, which had equally been confounded, sometimes with the last, but more generally with those which I am about to rank by themselves as properly deserving a separate place here. The differences are important, if the causes of both have sometimes been the same; for these are deposits formed in the present ocean, a portion of its bottom under a forcible change of place. They were those preparations for future strata which are still forming under the sea: and we may say that the time of their elevation has been anticipated.

Excluding then both these, there remain those which I shall now attempt to distinguish and explain: but though I must include them all in this chapter, it will be seen that they demand separate classes, as they are essentially different, under two leading heads; while, under other views, they may even demand a further subdivision. If, for want of sufficient facts, I cannot at present venture to divide them, future geologists may fill up, by examples, this proposed division; as they will hereafter, I hope, find no difficulty in furnishing the facts of which I am yet in want. The basis of arrangement is supplied in this theory.

If Lake Superior were now to be suddenly drained, we should find a series of strata covering its bottom; and from what we know of drained lakes, we should discover, at least, sandstones, shales, and limestones, together with loose clay, sand, marl, lignites, and terrestrial organic fossils, both vegetable and animal. Or, if it were to be filled up, through the deposits of its rivers, a similar series would occupy that cavity, which would then be a huge basin of tertiary strata. On a small scale, this fact is abundant in Scotland: we need not go further to seek for what may be called Lacustral fresh water formations. And this forms an intelligible division; demanding a class for itself, as far as the difference between fresh water and salt is entitled to claim two classes.

If we turn to the Caspian sea, and reason on it in the same manner, the same conclusions follow; and we may thus form a division or class of Lacustral marine formations. The qualities of the strata might possibly differ, or they might not; but the striking difference to a zoologist, would be found in the nature of the organic fossils. They would be marine, as far as such distinctions are secure and assignable. But another event would happen in this case: and thus would follow a distinction, the nature, and value, and causes of which have been strangely misapprehended, under that mixture of an ignorance seeking for causes where they were not to be found, and that love of the marvellous which has been far too predominant in geology. Where the rivers enter, the organized beings of fresh water would be deposited; and they would also be deposited under inundations, through transportation. It is the exact parallel to the æstuary of a river ending in the ocean; and thus would there be mixtures and alternations of marine and fresh water fossils, of animals and of vegetables. And further, as such lakes are maintained in a state of saltness, only because they have no exit, it is conceivable, from known facts, that such an exit might have occurred after a certain time; in which case a salt lake would be followed by a fresh one in the same place, whence also there would occur a change of the organic fossils in the collection of strata formed.

And in each of such cases, hypothetically put at present, there must occur purely terrestrial alluvia. The uppermost portion of a drained or filled lake must always be of this nature. But this is not all. These alluvia are progressive, and often at more points than one. And under this progression, they are subject to be overwhelmed by inundations, becoming thus buried under the finer materials which form the bottom of such lakes, of whatever nature. This fact occurs daily: and thus are terrestrial alluvia often deeply covered by materials which are properly aquatic, inasmuch as they form the subaquatic deposits. This is common with peat, often deeply covered by sand or mud, and thus even producing the alternations familiar in mountainous countries. In such alluvia also, there may occur the remains of terrestrial animals, whether from inundations or other causes; and hence may such remains alternate with mud or sand, or with those substances after conversion into marl, shale, limestone, or sandstone.

Now what I have thus stated hypothetically, consists of a series of facts, which, in some place or other, all occur. They are the facts on which a theory is always safely founded: it would be well if there were many as well supported. And they explain every essential circumstance which has ever yet been observed in such tertiary deposits as have demanded no other or accessary causes for their present condition. Except the occasional presence of gypsum, there is nothing unexplained, thus far. There does not even remain one of those imagined mysteries, respecting the mixtures or alternations of marine and fresh water fossils, or the occurrence of the skeletons of quadrupeds in such deposits, or the alternations of these and their loose alluvia with solid rocks. I have shown, in another place, that crowds of skeletons of land animals are sometimes deposited in the sea itself, and, in a former chapter, how rocks are produced from these loose materials. And thus may we dismiss, through a simple reasoning from the most common facts, all the wonders which have been attached to these basins, with their deposits and their contents, be those what they may.

Thus far, all is simple. There are tertiary strata, in basins, either of fresh or of salt water, in both cases Lacustral, in both containing terrene quadrupeds, in the latter case containing marine, and fresh water, or terrene, remains intermixed, and, in both, remaining in the very places where they were formed; remaining also undisturbed, and as easily assignable by any geologist deserving of this name, as the thousand more limited deposits of recently extinguished lakes. It were well if geological investigations were always as easy. That the deposits of Oeningen should have been so long mistaken, is a proof of the very scanty portion of observation and of reasoning which geologists have contrived to possess and apply to their science.

Thus we have a division of tertiary strata which will be single, if we rest it on the fact that it is *in situ*, or remains where it was formed; while if the quality of the water be held a necessary ground of further division, we must make two classes. I am, myself, inclined to consider it as one class out of the two into which I should divide the proper tertiary strata. Of the other class, the history and the theory both, are far more intricate; though I hope to show that the one which I shall propose is the true one.

The preceding chapter has indeed already suggested what this theory is to be; and I may examine its probability in the same hypothetical manner, first, and end, as before, with a reference to the facts in evidence.

Having shown that the "Subapennine" formations, are the bottom of the present ocean, thus elevated in times posterior to the general emergence of the secondary strata, it is, first, plain, that æstuaries must have been engaged in it, forming a portion of the total elevated mass. It is next evident, that as land already supramarine was further elevated in this case, it might have contained lakes of any nature, and in any stage as to the formation of subaquatic deposits. Thus might a pure basin of lacustral formation have been elevated by the volcanic forces; as this might also be either a fresh or, a saline one, or further, have contained that mixture already explained. And if there be such a basin, it may exist at any elevation equivalent to any one that has been proved, though not limited by such a measure. It might thus be situated as high as the highest of the Subapennine deposits, or as high as the coral reaches on the sides of Mouna Roa; not to make more extravagant suppositions, founded on the elevations of limestones in the more antient times of the globe. Under the same hypothetical view, if such a basin of strata can be distinguished from those *in situ,* the distinction should be sought in the neighbouring presence of such appearances as these of the Subapennine deposits, and in the disturbance of its regularity, under modes analogous to those which occur among elevated rocks in the older marine strata.

Such is the hypothetical view: a deposit of such a character ought to exist: the facts will show whether it does: and if it is not yet known, it will probably be found on due research; as this inference *a priori* will form the guide to the geologist, leaving him nothing to do but to observe. This is the true use of a work of this nature: it is the guide and the solution in one; for, if the views be just, it is Science; and all that follow, are artists only.

But there are no facts to this extent; for, excepting some very scanty records, well known, the confused and undiscriminating descriptions of geologists, numerous as they are, teach nothing on which we can rely. The fossil shell has had more charms than the history of the earth; and all that demanded and deserved observation has been superseded by petty details; easy and worthless, yet not without their poor fame, and thus, tempting, under the neglect of geology and the abuse of that term.

Yet we are not without evidence of some kind; and it is by reasoning on this principle, and this alone, that we can explain the two examples which have been best studied. If it does not therefore prove such a supposed case as I have stated, it does what is far more valuable; since it explains those examples which had never before been understood, and respecting which there has been as much wonder and mystery on the one hand, as there have been fanciful or false explanations on the other.

I allude to the deposits of Paris and of the Isle of Wight more particularly. The general history and nature of each is well known to all geologists; but the facts that concern us, here, are soon stated. The basin of Paris is elevated high above the present ocean, and it is insulated. I need here take no notice of its alternations; the possible causes have been already stated; and I but agree with many geologists of that country, in believing that there is none of that mystery which was stated in the original report. Under such a view it might have been an inland saline lake, but it might also have been an æstuary of the ocean. In either case, it is explained on the principle of elevation, under the reasoning applied to the Subapennine formations.

If this explanation be thought doubtful, in that case, there can be no doubt as to the instance in the Isle of Wight. The vertical position of the chalk is, here, an incontrovertible proof of elevation, of a partial nature, posterior to its original emergence. It is the case of Italy on a limited scale: and thence the conclusion is, that æstuaries, as well as basins, not only must have been elevated, but that one or both actually have been so.

Thus we not only prove the possibility of such greater elevations as I originally suggested, but are also provided with a solution for many cases; of which there may be more than this, and under many varieties, yet to be discovered. The condition of modern Italy proves that the relative level of the sea and land are changing, even in recent times; as its antient one, already recorded, proves far greater changes of the same nature. There is not merely variation, but vacillation of level; and I shall hereafter show that this is a common occurrence in many places. Thus then might tertiary deposits, whether in lakes or æstuaries, have undergone such vacillations, and to great extent, in those antient times in which volcanic action seems to have been more powerful than it now is: and, in this way, we find the means of explaining any condition of these tertiary deposits that can be imagined, and among others, such, for example, as a real alternation between a maritime æstuary and a fresh water lake.

Thus may I end this general view of the nature and distinctions of those two classes of deposits to which I consider that the term tertiary strata should be limited, to the exclusion at least of the others. Yet they evidently demand two terms and two distinctions. I cannot but believe that it will prove their true theory; and that, as far as the facts go, the exclusions which I have made will, together with these distinctions, bring light on this hitherto dark subject, and ultimately lead to such records of facts as will confirm them, while they complete this portion of the history of the Earth. I may proceed to notice some of the recorded cases, as being probably expected in a chapter on this subject.

Though careful investigations into this portion of geology, date only from the report of Cuvier and Brongniart, the first hint of such deposits seems to have originated with Lamanon in 1782, while those of Switzerland attracted the attention of Count Razoumowski in 1789. Of those which have been noticed, the following examples will serve to show that they form an extensive class of strata, to which future examination must be expected to add many more.

In Europe, there have been observed, those of England, comprising the Isle of Wight and the London district; those of France, including the basin of Paris, that of the Loire and Allier, that of the Garonne and Adour, with those of the Bouches du Rhone, and of Auvergne; to which may be added those of the Rhine. Similar deposits also occur in the North of Germany, from Holland to Poland; and in Bavaria, Switzerland, Austria, Hungary, Transylvania, Wallachia, Moldavia, and Bohemia. They have further been observed in the west of Italy, in Spain, at Salonica, near Corinth, and near Constantinople. In South America, they occur near Cumana, at Porto Cabello, Cartagena, and Santa Fé de Bogotá, as also in Guadaloupe; and many have also been named in Northern America and Asia.

But this is an indiscriminate mass, according to the views here entertained; and there is little reason to doubt that they contain examples of what I have included and what I have here separated. But it is not my business to analyze bad observations, or to do what the reporters have not; it is sufficient that I have shown, in the preceding chapter, an example of such analysis, and that the means of examining and arranging them are furnished by the present one. As far as it is safe to conjecture, those of Switzerland are lacustral and fresh-water deposits; those of Wallachia and Moldavia may be only æstuaries of the present sea, those of Italy are apparently the very strata described in the last chapter, and those of Auvergne may have been elevated ones; but whether basin-shaped or not, remains to be examined. Let it be remembered, that the interesting questions for geology are, not the nature of the deposits simply, and far less that of their remains: it is the mode of their formation, and their ages, especially as concerned with the last revolutions of the Earth; since that forms the great point of interest.

But if geology has hitherto occupied itself chiefly with petty circumstances, the pursuit of specimens, not of science, the facts on which science builds, it is true, but which are of no value without the building, thus, under its favourite bias to universal formations, has it also attempted to unite remote deposits of this nature under imagined resemblances or identities. Unclassed as they have remained, the futility of such attempts is obvious; but even where the geological facts correspond, there is no philosophy in such endeavours. If there are any strata on the face of the earth which are truly independent, it must be these. General reasoning, and analogies drawn from the older strata, particularly from the coal series, must conclude, that such deposits will differ in different places; and thus the fact proves. Not only do the number and the succession of strata differ in each, but the natures of the substances themselves. Hence also the impropriety as well as the inconvenience of using a positive term, such as that of Plastic clay, or Calcaire grossier, to designate what, in another place, may be neither of those particular substances. It is plain that the nature of any such deposit must have been regulated, like every anterior stratum, by the matters which were present: and to suppose that the organic remains could correspond in remote places, is even to confound all zoological history.

If this remark applies to those which are of marine origin, still more is it true where they have been formed under fresh water; since a want of coincidence or similarity must still more occur in such cases, and especially, as far as plants and animals are concerned. It is this practice of generalizing from a particular tract or country, which has constantly obstructed the progress of Geology, obstructing it still, more than ever, in the case of the secondary strata; as the evil effects increase, the higher we ascend in the series, from the increasing independence of the deposits. There is no chance of our becoming truly acquainted with the deposits under review, till Geologists shall substitute real observation and description, for a convenient phraseology; convenient, as saving the trouble of observation and reasoning, equally. There is some truth also in Breislak's satirical remark, that The London clay, The Muschelcalk, and so on, are but modes of national vanity.

I shall quote the two last described examples, those of Paris and the Isle of Wight, as specimens of actual details, and as examples of differences under analogy, these being also more instructive on this last subject, on account of their proximity. But I must condense, and refer to the well-known original reports; premising, however, that these are by no means always free of system.

In the basin of Paris, the lowest strata consist of sand and of clay, with marine remains, followed by a deposit called the Calcaire grossier, including many subordinate strata, and series of strata, as they are considered, consisting of limestone, marl, sandstone, chert, and shale, and containing also marine shells. This is, evidently, a marine deposit, and is called the first marine formation; but, in the upper parts, it contains fresh water shells, and Lignite. Above this, another series has been constructed out of certain strata of limestone, and of calcareous and argillaceous marl containing gypsum; including also, marine shells and fresh water ones, lignites consisting of the palm tribe, and the bones of quadrupeds and amphibious animals. This series has been called the lowest fresh water formation. Still higher, follows marl containing marine shells and remains, sandstone and sand, and a cavernous chert, or the millstone; forming what is called the upper marine formation, though it is a question whether this last rock does not belong to the fourth series. That is a calcareous rock containing fresh water shells, of species still existing, together with vegetable fragments. The whole is followed by the alluvia, which, however interesting from their interred remains, do not concern the present question.

In that of the Isle of Wight, the clay is succeeded by various marls, including fresh water shells of different species and genera, and, subsequently, by other marls containing marine shells, followed by strata of calcareous rock, marl, clay, and sandstone, in which the shells belong to fresh water. Thus, here, as in the basin of Paris, it is conceived that there are two deposits from fresh water, separated by one of marine origin.

I shall leave it to others to apply to these the critical analysis of which I have furnished the grounds: and still more shall I do this as to the London deposit, on which the remarks ought to be obvious enough. It is one of the cases demanding a decided separation; as are many more of those formerly enumerated. To reason from the observations of others, is always sufficiently disagreeable; when the observations are bad, it is often a hopeless attempt. When such a deposit as that of Oeningen has been called marine, because an obscure shell or doubtful fish was supposed to be such, it is a sufficient example of the hopelessness of such attempts. At any rate, it is not the business of a systematical work.

The general conclusion derived from these and the other deposits, as it regards their strata, is now obvious. Clay and sand must have been deposited in these circumstances, as it has been in prior states of the globe, and as it is now; and calcareous matter must have been produced by marine shells; as, lastly, these strata must have often been indurated into rocks. Sand, clay, marl, sandstone, shale and limestone, are therefore the fundamental strata of these deposits when marine; and when fresh water strata succeed, they can be but modifications of the same substances, while the coal series proves that all these rocks are formed under such water, as does the travertino of Italy. The sources of the organic fossils are no less obvious.

But I must not pass from these, without inquiring into their value in determining the marine or other nature of these strata, This is especially necessary; as the theory, and the mistakes of fact, together, have been among the chief sources of erroneous judgment in these cases, and will remain so as long as this engrossing pursuit shall occupy all the attention of geologists, and this hypothesis shall continue to rule. If to mistake respecting a fish has been sufficent to confound the case of Oeningen, it is easy to see what more may have happened and may happen again; not only in such instances, but in the judgments respecting alternating deposits.

I do not give catalogues of species and genera. These concern zoology, not geological science: it is a pursuit even more separated from the latter than mineralogy; and a geological system does not encumber itself with catalogues of minerals. I shall only therefore name, among the living genera of fresh waters, Lymneus, Planorbis, Physa, Paludina, Ampullaria, Cerithium, Melanopsis, Melania, Nerita, Cyclas and Unio. Of these, Lymneus, Planorbis, Physa, Paludina, Cerithium, Melanopsis, Melania, and Nerita, are found in the fossil state; and Paludina, Ampullaria, Cerithium, Melania and Nerita are common to fresh and salt water. Of the shells called exclusively marine, Modiolus, Mytilus, and Corbula, live in fresh water; and different species of Anodon, Cyclas, Unio, Tellina, Cardium, and Venus, some belonging to fresh and others to salt water, are found promiscuously in the gulf of Livonia. Our own muscles and oysters, and many more, thrive better in fresh water than in salt; and reversely, many fresh water shell fish can live in salt water, as those of salt marshes are especially indifferent on this subject. Independently of all these as grounds of doubt respecting such decisions, there is too much uncertainty respecting the former states of the globe and of its waters, to permit us to decide positively on the nature of a stratum, from its fossils. Still less can that be done by means of two or three species; nor are we by any means sure that our anatomical arrangements of living species as to this point, can be extended retrospectively. If also we consider the repeated changes of the surfaces, particularly of sea coasts, the contests between salt water and fresh in æstuaries, changes in the character of lakes in this respect, and accidental transportations of shells themselves, it is plain that there will always be danger of error in forming absolute rules, and, still more, in attempting to judge from a small number of circumstances or a limited number of species.

The difficulty is much increased where fossil species are concerned, by the imperfections of their characters. The minute parts on which the distinction so often depends, such as the teeth of the hinge for example, are frequently injured or destroyed. With respect to tenderness and delicacy, or the reverse, I may quote De France and Ferussac, as opinions that there are no such general distinctions of character. It is all matter of experience, or of empiricism; and that experience also is limited. Nor can we decide that an antient genus or species, now belonging to fresh water, might not have been marine, or the reverse.

This same difficulty exists with regard to the fishes; by the remains of which naturalists have so often pretended to decide on the character of a deposit. As the facts which prove this are important, and as they are new to geology at least, I shall quote them from my former writings. They will show that while many fishes change their residence voluntarily, others can be compelled to do so; and that marine species will not only thrive, but breed in fresh waters, under compulsory change.

That this fact was known to the Romans, is proved from the writings of Columella; though the species of sea fish thus transferred to fresh water can not be fully ascertained. This practice descended to Sicily, where it is now in use as to the Mullet and Lobster. In nature, a certain number of fishes are either migratory between fresh and salt water, or else permanently indifferent to either. Such are the Sturgeon, the common and the grey salmon, the gwiniad, smelt, salmon trout, and Salmo migratorius, together with the Lamprey, stickleback, Eel, and others. The Mullet, conger, torsk, sprat, shad, cottus quadricornis, rockling, whiting pout, mackerel, herring, Delphinus leucas, and a few more, also quit the sea for fresh water, either for the purpose of spawning or for other reasons; while the Cod, in Shetland, is the voluntary inhabitant of a fresh water lake. Two or three species of flounder also abandon the sea to live entirely in rivers. In a reverse manner, the pike, and eight species of Cyprinus, which elsewhere are inhabitants of fresh water, abide permanently in the Caspian. Further, the salmon trout, supposed necessarily migratory, and the herring, purely a sea fish, reside permanently in fresh water lakes. The process of naturalization, conducted under my own directions, has moreover shown that the Turbot, sole, plaice, mullet, smelt, atherine, horse mackerel, pollock, loach, basse, rock fish, whiting pout, rockling, prawn, crab, and stickleback, may be habituated without difficulty to fresh water; while all those which have hitherto had time enough, have bred and propagated.

Such examples render it probable that most, if not all fishes, are indifferent to the quality of the water, provided they can find food: and when it is considered how many changes the surface of the earth may have undergone, it is evident that no judgment respecting the nature of a deposit, can be derived from a previous decision respecting the imagined habits of fishes as to the quality of the water in which they may have resided.

What is not less important in a geological view, every species on which this trial has yet been made, suffers alternate changes from fresh water to salt without inconvenience; having been retained in a pond where these alterations take place. It is easy to see how these facts must affect our decisions respecting the nature of marine and fresh water deposits as far as that is to be determined by the remains of fishes. I need here only add, that as far as the question of respiration is concerned, there ought to be no difficulty in the change from salt water to fresh, since the adhesion of the oxygen is less strong to the latter than to the former.

But, from the fact that the Pike and the Cyprini prefer the Caspian when they might remain in the Wolga, it is not improbable that many or all fresh water fish might also endure the sea permanently, as the Salmon and others do, interruptedly. And though, in antient times, fishes had only been migratory, as they are at present, it is plain that the fossil remains of those animals can never aid us in deciding on the character of a stratum. That many of the fresh water amphibia now frequent the sea, from choice, is known respecting the alligators of Cumana, and those of the Pellew and the Bahama islands. In a reverse way, the Turtle of the West Indies has been taken alive in the fresh water of the Tamar, whither it had wandered of itself, after a residence of unknown length. That it will also reside for a long time, and continue to thrive, in fresh water, has been proved in Kent.

If my own views as to this subject are now terminated, I presume that I ought still to notice one of the theories that has been entertained respecting it; as it might still possess an injurious weight if not thus examined, though the answers might easily be derived from what has been said. They who speak of retreats of the sea consequent on its diminution, are but offering that gratuitous and untenable hypothesis which so long weighed down geology as to the older rocks. As far as is necessary, these dreams will be examined in a future place. Any other retreat would only explain the alluvial deposits already distinguished. I shall sum up this chapter with a brief statement of what I consider an arrangement for all the strata later than the great marine series.

If no general revolutions have occurred since the great marine elevation, the facts of the last chapter prove that there have been great partial ones, dependent on volcanic actions. By these have æstuaries, marine basins, and fresh water lakes, been elevated; while the volcanic matter needs not be present, since it is not always so in the Coral islands. And these, being marine, or fresh, or alternating, will form the more interesting of the tertiary formations, separated from the ocean and elevated above it.

Marine or fresh water lakes have been drained or filled; and, in this case also, there may be alternations. Such completed deposits may have also been disturbed by volcanic elevations, thus associating them to the last division. And this is the second, and only other division of the proper tertiary strata: unless as we subdivide it into marine and fresh. This division also is of any age, from the most remote times of the emerged land to our own day: but the former is exclusively antient.

These comprise the more rigid tertiary formations. But the association between the first class and those described in the last chapter is intimate, as they may not also be often distinguishable. In the mean time, the cause and the facts being proved in this case, and not so clearly in the other, it may be as well for the present that they should be kept separate.

The alluvial æstuaries of the present ocean, however distant from the sea now, must be entirely rejected and referred to the alluvia. To distinguish these, there is required, in practice, that geographical tact and knowledge which have distinguished leaders of armies, but rarely geologists. They seem to have forgotten its necessity. In the account of Glen Roy, I have given a practical illustration of that indispensable necessity in the explanation of Geological appearances.

We finally see how the facts of the last chapter, bearing on the present, explain the mysteries of the last revolutions of the Earth, and that they are but the continuation of the former actions on which everything depends. The mysteries have been created by Geologists who had formed no conception of such a cause, and who had never extended their Geology beyond the upper strata and their remains; disturbing alike themselves and their readers, by vague notions which they could not even express, and tormenting themselves with their own visions.

CHAP. XVII.

On the Formation of Coral Islands.

The production of the Coral Islands of the great Pacific ocean, which endanger this navigation and that of the Indian Archipelago, and are tending fast to destroy that of the Red sea, is a fact completely distinguished from all other subjects of geological investigation. It also forms a most interesting and necessary branch of the present inquiries; and it is the more indispensable to examine it, because it has hitherto been unaccountably neglected by geological writers. In the case of other submarine animal formations, the results are limited to the germs of future and far distant continents, as the works are without apparent design. But the operations of the coral animals are very different. By their own efforts, assisted by some incidental causes, they build their works above the level of the ocean during their own lives; thus forming rocks and islands, without the necessity of those actions which have raised all the other submarine strata from below. In this manner is the habitable surface of the earth extended, and new regions arise in the ocean. The silent and unnoticed operations of the minutest animals of Creation, are daily preparing the foundations of land, beneath it, destined to extend the dominion of man over a far wider range.

But the volcanic agencies which form the subject of a former chapter, are frequently found to succeed to these; thus accelerating their results, elevating high above the water that which would otherwise have for ever remained but little raised beyond its level, or producing, in the course of a few days or weeks, what might have required the labour of centuries. Geology therefore finds two distinct sources of interest in the study of these islands. If the proofs which they afford of elevating forces, connected with volcanoes and acting beneath the surface of the earth, are valuable, the simple history of the Coral islands, independently of this interference, is scarcely less worthy of notice. It cannot at least be less interesting to study the formation of immense masses of calcareous rock by living animals, than by the accumulation of the spoils of dead ones. It is, in many respects, even more so; not less from the illustration which it affords relating to the antient calcareous rocks of the globe, than from the tangible nature of what, in these analogous cases, is only matter of inference, and from the comparative feebleness of the agents concerned in the production of these important effects.

With respect to all the organic fossils, their chief interest is derived from the relations which they bear to the existing species, and from the effects which they have on the structure of the earth. We are surprised at the immense accumulation of the shells which form the secondary calcareous strata, and with the enormous additions which the earth has received from the labours of animals, generating mountains out of the habitations which they had formed for themselves. Yet these results rarely strike us; as the very fact indeed has been doubted or denied. It can at least be denied no longer; for it is before us, if under another form. They do not strike us, because we see these rocks long deserted by the sea, associated with others, and without traces of a living origin except to the eye of a geologist. We contemplate the surface of the earth as so much rock, simply; but when we see the very work itself, and the steady increase of the land through the labours of the coral animals, the importance of this race of beings becomes sensible, and we are struck with the immense influence which all the hard marine tribes must have possessed in the formation of the present earth.

In the case of shell fishes also, we can only infer, that the present extensive masses of limestone have been produced by their labours. There is a complete chasm between the labour and its produce: but in the coral formation, this is filled up. The labours of the shell fishes are concealed by the ocean, never to be known to the geologists of our own earth: the works of the coral animals attain the surface of the sea. The strata which they form are at once living and fossil; we see them in the act of production, and the mountains grow up to the day before us, new parts of our own earth, not mere preparations for a future one.

It is sufficient here, to speak in the most general manner of a tribe of animals, for whose description, works on Zoology must be consulted. In a popular view, a coral is a calcareous structure, inhabited by numerous small animals, or polypi; and each form of coral possesses its own species. Each, therefore forms a sort of colony; the inhabitants of which are disposed in minute cells, which they construct themselves, thus producing the general structure by their joint labours, as if all actuated by one design and one mind.

This is the obvious appearance. But, in reality, the entire coral plant is one animal. A continuous animal structure pervades the whole; and the calcareous matter, in whatever form, must be viewed as the shell; being a secretion or deposition of earth in its substance. And though in one sense this may be viewed as an original colony, all the polypi are but parts of one whole, as flowers are of a plant; while the resemblance would be perfect if the flowers were to produce the plant instead of being produced by it.

This analogy, and the general nature of the corals also, will be made more intelligible by examining the Vorticella, possessing an essential resemblance to the coral polypus in everything else but the production of a shell. The simple vorticella swims at liberty; resembling a flower-bud, and consisting of a body provided with tentacula. Other species are fixed by a pedicle which restrains their motions within narrow limits. In others again, two or more are united by one stem; and, at length, we find highly ramified kinds in which each branch is terminated by the active polypus. In this case also, each polypus is partially independent, yet all depend on the whole; presenting a singular analogy to the vegetable identity, especially in such cases as that of Epidendron, where the leaves combine to produce and nourish the plant; so as to give a sort of colour to that gradation so often sought by visionary naturalists, between one department of nature and the other. If the corals differ from the vorticellæ in the secretion of a stony matter, so do they present an analogy to the Chara and the Coralline, secreting a calcareous bark, and the Equisetum, which produces a siliceous one. The madrepore will serve to explain more distinctly the mode in which the calcareous cell is constructed. This polypus is surrounded by feet, or hands, surrounding the body and divided at the extremities; each embracing a lamella of the star. As in the

Hydra and Actinia, the tentacula are the organs of feeding.

The whole of the species of corals engaged in the formation of the coral banks, are not known, but a considerable number of genera and some of the species also have been ascertained. The Madreporæ appear to be among the most numerous; Milleporæ, Tubiporæ, Caryophilleæ, Distichoporæ, Corallinæ, and many more also abound; while for these I must refer to the works on this subject. Echini, and numerous other shells which reside on the banks, serve to augment the general mass of calcareous matter. Careless observers have mistaken Holothuriæ, and other soft worms for the coral animals.

It is for geography, not for a work of this nature, to describe the islands and rocks produced by the Coral tribes. It is here sufficient to mention the islands to the south of the equator, between the west coast of America and New Holland, crowding the whole of that sea, under a rapid increase, accompanied by still more numerous rocks, destined to become the seats of vegetation, and the habitations of man; and perhaps, at length, to form a continent in the Pacific ocean. To these, abounding particularly between New Holland, New Caledonia, and New Guinea, I may add those of the Indian Archipelago, including Cosmoledo, Chagos, Juan de Nova, Amirante, Assumption, Cocos, and the Maldive and Laccadive islands.

When we consider the feebleness of the means and the minuteness of the agents, the extent of these reefs and islands is a subject of equal curiosity and surprise. For these however, the reader may consult the writings of navigators: though it will indeed be sufficient to cast an eye over a map of the Pacific

Ocean. I may now however name one or two examples. Among these, Tongataboo is twenty leagues in circumference, and is elevated ten feet above the water. It would have been desirable to know the thickness of this mass or bed of rock, but the soundings have not been given. These are, however, known to be deep, throughout all this sea; since they often exceed two and three hundred fathoms: so that on any view the whole constitutes an enormous bed of organic limestone. But the largest yet described is the great reef on the east coast of New Holland, which extends in an uninterrupted course for three hundred and fifty miles; forming, together with others, a nearly continuous line of one thousand miles or more in length, varying from twenty to fifty miles in breadth. To form a just conception of such a production, we should imagine it exposed from the foundation; it is a mountain ridge which will bear comparison with many of the larger tracts of terrestrial limestone in height; as it far exceeds any that are known, in the extent of its range. To him who had not known such a production, viewing only the coral itself, or the labourers in this work, it would be incredible. It would be interesting to know the height of this ridge from the bottom; and hereafter perhaps, some navigator, informed of that interest attached to this subject which did not exist in the time of Cook, or Flinders, or even of Kotzebue, will enable us to form some estimate respecting it.

The reefs, which are the germs of the islands, are often disposed in a circular manner, or in other curved or oval shapes: and I shall presently show that this disposition is one of a singular ingenuity, as it respects the form and character of the future island. In other cases, they are simply grouped, without apparent order; though it may be suspected that there has here been a deficiency of observation. Lastly, they are found extending in long lines, more or less straight, or in rows.

There is some reason to distrust the assertions that have been made respecting the rapidity of their production. It is easy to mistake one reef for another, amid such crowds, and in seas so little known, where also there is no guide but the ships' reckonings and observations. And it is obviously an excuse for the error of a navigator or an accident to a vessel, for a bad reckoning or an incorrect chart, to find a new rock where the old one had been forgotten or misplaced. If there is one evidence of an unexpected rapidity of formation, given by a recent navigator, who, after three years' absence, found some parts of a reef which had scarcely reached the surface at his former visit, clothed with vegetation, it will be immediately seen that the last part of the growth of a reef does not depend on the animals themselves; so that it proves nothing.

There is reason to believe, from a considerable examination of the soundings in this ocean, that both the places of the coral reefs and their peculiar dispositions, are, very often, if not always, determined by the forms of the bottom of the ocean where they lie, and that they are placed on the submarine hills of these seas. When disposed in straight or curved lines, the windward side of the structure, which is exposed to the breach of the sea, rises almost vertically, like a wall, reaching the surface; while, to leeward, they shelve away, deepening the water in this direction. It is this windward abruptness, in particular, which renders these rocks so dangerous: since there is no warning through alterations of soundings. But the remarkable fact is the utility of this proceeding: since the shelter produced to windward enables the work to go on in the opposite direction, with comparative facility. If the instinct of working upwards to the surface is remarkable, this is even more so: while, in all we trace the evident intention, in that Power which gave these instincts, to effect the very ends that are attained.

Some peculiarities occur in the circular groups, which are particularly deserving of notice, from the results by which they are followed, and to which I have already alluded. The first appearance consists in a number of detached rocks and islands, forming a sort of chain; while, in further progress, intermediate ones spring up, so as gradually to unite the whole into a continuous line or circular form. On the outside of this line, or ring, the water is deep and the walls vertical; but, within, it shoals in different places, in a sort of general declivity from the margin. Thus it produces a solid mass, of a shape like that of a plate or elevated basin; being a kind of platform with a depressed centre within a vertical wall. In the smaller circles, when this process is completed, the reefs bear this resemblance to a circular basin very accurately. And this cavity is, at first, a kind of salt lake; becoming shallower as the animals, still acting within it, prolong their works upwards. And thus it gradually becomes so contracted, that the fall of rain is sufficient to freshen the water; whence follow the death of the animals and the cessation of their operations. Thus it remains a cavity, and becomes a lake; forming a supply of water for future inhabitants, under an arrangement no less remarkable for its foresight and contrivance than all else which appertain to these wonderful structures.

It must now be observed, that though the windward side is vertical, it cannot become an effectual protection against the sea, for the leeward parts, without a further set of operations acting in aid of the original process. The real dam, or pier, under the protection of which the whole mass extends, is produced by the fragments of the corals. As the animals cannot live out of the water, they cease to grow whenever they have arrived at the high water mark, and the proper animal structure is therefore terminated at that point, for ever; but at low water, the reef is above the sea, and thus becomes exposed to the force of the waves, which breaks off the upper parts and washes them to leeward, where they collect; while the animals, on the windward side, keep up a constant supply of materials. In this way, the fragments of coral, and the sand produced by their attrition, become a species of alluvium on the surface of the living coral rock, which is gradually cemented through the action of the water on carbonat of lime, so as to produce a species of oolithic limestone, resembling that so well known in the West India Islands. Thus does the whole platform gradually become elevated above high water mark, as the outer dam had at first been; until it forms a continuous surface, aided in the regularity of its levelling, for a considerable time, by the breach of the sea. And the effect also of this process is to produce regular strata: while fragments of these, being often large blocks of stone, are frequently piled up on the outer ridge, and even further, so as to add to the total elevation, and consequently, to secure additional protection from the sea. Loose shells, breeding among the coral, and washed up in the same manner, add to the general height and solidity of the fabric. The same process is also going on during this time, in the interior parts, where the projecting banks lie, so that all these at length extend and unite; producing, according to the original numbers and groupings, islands of various sizes, and often of great magnitude. Occasionally, the lakes before mentioned are also filled up by the growth and decomposition of vegetables, as in other similar circumstances.

To clothe these islands with soil and vegetation is all that remains. This is a more rapid process than would be expected, as I have already remarked. The first foundation of it is laid in the sand produced by the destruction of the corals; and as sea plants take root and grow upon it, this becomes a vegetable soil. Sea birds of various kinds then find a residence, continuing to add to this soil; and at length the floating seeds of various plants are arrested, when a terrestrial vegetable covering succeeds. Hernandia, Morinda, Cerbera, Scævola, Pandanus, are among these plants, the natives of all the islands of this sea.

Such are the uninhabited islands, for the process is now completed. How man finds his way at length, I need not say; and how he has filled many of these islands, is well known.

But far more is yet wanting to explain the present state of many of these islands; and here commences a new source of geological interest, of a very different nature. It is plain that under this mode of construction, they can have no great elevation; and accordingly, the flat ones are scarcely raised more than five or six feet above the level of high-water mark. But as many of them are far higher, we must resort to some other principle for effecting this purpose. And this is the action of a subterraneous elevating force. How this, and its effects, have been overlooked by all the scientific navigators among these islands, it is not for me to explain: how geologists in particular have neglected that which offers more demonstrable and tangible assistance in explaining all the revolutions of the earth, than any thing which exists, they must themselves say; but how it is connected with these, and, more immediately, with the elevations of Italy, it is my business to show.

I have already said that Tongataboo is ten feet above the high-water mark; and even this is a greater elevation than can be produced by the action of the sea; supposing too that all the surface consisted of fragments, and not of perfect corals, unable to rise a single inch above the highest level of the water. But in many of these islands, the corals, with all their characters perfect, are found at elevations many hundred feet above this. I need not say that the ocean could not have been depressed by this quantity, or could not have stood so much higher than it does at present. To suppose this, is for those who dispose of oceans at their pleasure. The island itself must have therefore been elevated. But independently of this reasoning, there are sufficient proofs of such elevation visible. And the causes to which this is owing, are easily traced, however they may have been neglected. They have acted on the bottom of the ocean, so as to have effected the changes necessary for the production of these results; and they have consisted in volcanic powers. In many places, there is no difficulty in discovering the very fact, or the cause; but the following islands afford the most convenient and satisfactory proofs.

If I take the Tonga islands as a convenient case for this illustration, Tongataboo and Eeooa form the first link in this chain; and it is one which is peculiarly valuable, from the great differences between the forms of these two tracts of coral land under such a proximity. Eeooa, distant from the former only twenty miles, consists of a hill of considerable elevation, though that height is not given. But for the present purpose, this omission is not of any moment; it is sufficient that it displays coral three hundred feet and more above the level of the sea, indicating, in the most demonstrative manner, the former existence of a force which must have raised it to that height. It is also probable, from the proximity of these two islands, that both were raised at the same time, and by the same force; the chief power having been exerted under Eeooa, while the lower island of Tongataboo was raised to a height, comparatively so inconsiderable, because it lay on the boundary of the elevating power. There is no other cause adequate to the production of these effects; as, of the effects of such causes, I have given ample proof in a former chapter: and it must be superfluous to say that if such a power produced the greatest of these effects, it is also capable of accounting for the least of them.

It is true that there is no volcano actually existing in Eeooa; but if it can be shown that this force has exerted its action in a manner so demonstrable that it has actually erupted volcanic matter, or if there are volcanoes now in existence in the same seas, and with the same consequences, even to the existence of coral high on the sides of the mountains, the inference is perfect; while the least degree of elevation, as now described, is precisely what happens where earthquakes attend the eruptions of volcanoes, situated often at considerable distances.

It is possible that the volcanic power may, in the particular case above named, have been exerted under Eeooa itself: the nature of the summit of the hill is not described by Cook or by Mariner, as both have neglected to notice volcanic rocks now known to exist. But whether this be the case or not, the presence of a volcano in this group is established. Toofooa contains one which is always burning, at a distance of only seventy miles from Tongataboo. And as the small island Kao, about three miles from Toofooa, is a cone; it is probably of the same nature.

It is indeed plain that a volcanic force has been exerted very extensively in this part of the Pacific Ocean. Of the Friendly Isles, of Cook's arrangement, thirty-five are hilly; and so are Otaheite, Bolabola and Eimeo. Though he has not mentioned volcanic rocks, they occur in many places: there are three burning volcanoes in the Friendly Isles: and Eap, which lies to the west of the Caroline Islands, is a seat of volcanic force. According to Kotzebue, earthquakes are here frequent and violent. When Ulea trembles, all the Coral reefs in the vicinity are shaken. In the North Pacific also, coral is found on Owhyhee, far above the sea; and in this island, Mouna Roa is a volcano, as all the rest of this lofty mountainous group is formed of volcanic rocks.

These facts complete the chain of evidence in a manner so satisfactory, that it is unnecessary to enlarge on it. But the elevation of volcanic islands in other seas illustrates and confirms these reasonings, as does the history of the Italian alluvia, which, itself, receives illustration from what has now been described. In the same way, the changes in the level of the land adjoining to many well-known terrestrial volcanoes which have been accompanied by earthquakes, serve to establish the truth of this explanation; could any further confirmation of a truth so obvious be required.

In terminating these remarks on the Coral islands, it will not be uninteresting to observe that analogous appearances occur in the volcanic islands of the African coast, and elsewhere. Secondary limestones are found lying upon the rocks which are the produce of fire, containing marine remains, yet elevated above the surface of the ocean. If the elevation of these strata, abstractedly considered, should be thought to prove nothing more than what may be inferred from the analogous appearances all over the world, it must be recollected that there is here present, not only an obvious and active cause sufficient to raise them from the bottom of the sea, but that the actual exertion of that power in analogous cases, is proved by the phenomena of the islands now described. Owing to a higher relative antiquity, or other causes, there may be differences in the results, or in the present appearances; but the strength of the general argument derived from them remains undiminished. The Bermuda islands, as far as their structure can be inferred from the descriptions of persons, not geologists, present confirmation still more complete. They are formed of "coral rock and limestone containing shells;" and the former, or the corals, are found at the higher elevations, the greatest altitude of the land itself being estimated at three hundred feet.

Since these observations were originally published, they have been confirmed by Moreau de Jones, in the West Indian islands. The facts there visible prove that the calcareous strata have been elevated by the volcanoes of those islands; which have also, in some parts, broken through them, while, in others, the volcanic matter is still covered and concealed by the limestone, which thus forms the surface of that hill, of which the interior is an igneous rock. It is an unexpected confirmation of these general views, and independent of all geology or theory, to find the same belief among the people of Banda; who, according to Reinwardt, assert that their island is still, visibly, rising.

There remains a chemical question respecting the generation of coral islands, which is extremely obscure, while it bears equally on the formation of the ordinary stratified limestones. This is the production of the Lime: but chemistry has, hitherto, not a rational suggestion to offer. The fact of which we are sure, is, that the carbonat of lime is formed by animals, in whatever manner. And if the great masses of coral rock thus described be estimated, it will be seen that they are equivalent to some of the largest deposits of secondary limestone existing. That the secondary limestones have been produced by the animals whose shells are still imbedded in them, is far therefore from being so unreasonable an opinion as it has sometimes been considered. It is not compulsory to believe that all limestones have originated in the same sources; but when we recollect that these rocks abound among the secondary strata, while they are comparatively rare among the primary, diminishing in quantity in proportion as we recede from those periods in which the earth was most fully inhabited, we contemplate a fact which confirms the deduction that may be drawn from the appearances now described.

CHAP. XVIII.

On Volcanoes and Earthquakes.

The history of Volcanoes forms one of the most popular and the most interesting branches of Geology. If the splendour and the consequences of their eruptions attract the ordinary spectator, the Geologist finds, in their phenomena, difficulties which it is his duty to explain, and analogies widely connected with other departments of his pursuit. While they are the great sources of the rocks that are formed in our own times, they throw light on the history of the unstratified substances: in the power by which they elevate and derange the surface of the earth, they afford a clue to the nature of the far greater changes which it has formerly undergone: and, in reflecting on their seats and causes, we are led to form conjectures respecting the interior parts of the globe.

If, in history as in nature, we find the memorials of countries destroyed, of mountains formed and demolished, of cities overwhelmed, of rivers that have changed their courses, of lakes swallowed up, or generated, so, in compensation of all this evil, the land is raised above the level of the waters and new islands emerge from the ocean. The most delightful parts of Italy have suffered from volcanic fires; the history of the Subapennine hills even proves that vast tracts of this country have been raised into existence by their power. And, however terrific may be the phenomena of volcanoes, they are less interesting than these records of antient eruptions; which connect the present appearances with others, of a far more distant origin, explanatory of the still anterior revolutions of the Earth.

As might be expected, few subjects have produced more writers. But we search in vain through the great mass of their works, for useful or scientific information. Even the scientific investigators, anxious to maintain some favourite theory, and educated in prejudices respecting those rocks in nature which bear the greatest analogy to volcanic products, have rarely done justice to a subject, of which the principal obscurity will probably be found to arise from their own peculiar views.

Of the Geography of Volcanoes.

However numerous the existing volcanoes may be, their extent and numbers have once been far more considerable; as is proved by the extinct remains found in many parts of the world. The great age of some of those has been inferred from their rocks being, in some places, covered by strata of secondary limestone. But the nature of this fact has been mistaken; as these strata have been formed beneath the sea, and elevated by the volcanic power. It is the very case described in the last chapter. Such volcanic rocks are therefore not necessarily older than the traps, because they are beneath the strata while the latter are above them. On the contrary, the reverse may be inferred; as time has carried away those strata which have unquestionably covered the trap rocks in many places, while those which thus lie on the volcanic products, remain. If geology could be proved to have reasoned worse on one subject than another, it has been on this one. There are doubtless many differences of age among extinct volcanoes: but it is difficult, for want of explanatory facts, even to approximate to the period of their existence or extinction, far less to determine their absolute or relative ages. This difficulty is materially increased by the difficulty of distinguishing between the traps and the volcanic products, where the traces of volcanic action have disappeared. Where these eruptions have been submarine, the difficulty is insuperable; as both classes of rock are then under the same circumstances, and are, in truth, identical. These are the difficulties which yet beset the long agitated question of the Euganean hills and the Vivarais; and which will be found to exist in many other cases. With respect to Italy, it is certain that many of the volcanic appearances are prior to that remote period when Europe was inhabited by races of animals long since extinct in this continent. In other cases, history offers some aid; recording the activity of many which have been long dormant. The Scriptural record of the destruction of the cities on the Dead Sea, is the last information which we possess of the former activity of that volcanic district.

The extinct volcanoes of Auvergne are among those which have most excited the attention of Geologists, from the peculiarity of their positions and appearances. In Italy, the traces of this nature are innumerable; sixty extinct craters being reckoned by Breislak between Naples and Cumæ. Many other parts of this country, and of Sicily, present the same records; which occur also in the Lipari isles, in Elba, Sardinia, Ischia, Procida; as well as in Lemnos and other parts of the Greek Archipelago. St. Helena, the Azores, the Cape de Verde islands, and Madeira, present similar appearances, as do Java and other islands of the eastern seas. Iceland is one entire volcanic mass, of which a very small portion is now in a state of activity. In Peru, there are abundant traces of the same nature; but I need not enumerate here all which have been recorded.

The volcanoes now in a state of activity, are often found in the same districts or places as those which have been long extinct; indicating the existence of a permanent cause, and a tendency to the repetition of the same phenomena after long intervals of repose. Those of Europe are limited to Iceland, and to the Italian volcanoes; too well known to require enumeration. In Asia, there is one extensive volcanic tract; twenty being enumerated in Kamtschatka, of which five at least, if not seven, are of great dimensions; the whole appearing to be connected with those of the Kurile islands, which amount to nine. In central Tartary there are two or more, among which Tourfan is conspicuous; and there are some in China which are yet unknown to us. Kæmpfer has counted eighteen in Japan and the adjoining isles; and there are nine in the Ladrones, with many in the Philippines; the principal island, Luzon, containing three. The Molucca islands also abound in them, as do Sumatra and Java; the latter, according to Raffles, possessing forty-eight. Many others occur also in the adjacent islands.

Among African volcanoes, there are said to be forty-two active or dormant in the Azores. In the Canary islands, that of Teneriffe is particularly noted; and in the Cape de Verde, Fuego is active, as are those of Ascension and Bourbon; but the interior of that vast continent is nearly unknown to us. The occurrence of obsidian in great abundance on the shores of Arabia, must be taken as an indication of unknown volcanoes in that country.

Those of America are among the most celebrated. On the North-west coast, three were observed by Captain Cook; and others exist on various parts of the coast between Alaska and California, in which last district there are five. Along the Cordilleras of Spanish America, they are found dispersed and intermixed with extinct ones, from the Tropic of Cancer to Terra del Fuego: and among the latter, Chimboraço is noted for its great elevation. Here, they seem to extend in rows nearly north and south; but, in Quito, they diverge from that line, being spread over a space of seven hundred square leagues. The Cordillera is also intersected in the latitude of 19° by an east and west range of them, containing the recent volcano of Jorullo. The chain of islands which extends from Alaska, contains seven in a state of activity, and they are also found in the Gallipagos. In the West India Islands, St. Christopher, St. Vincent, Guadaloupe, and Nevis, are still active, and many extinguished ones are also found. I have but just noticed those found in the Pacific ocean, and may refer to Arago for a more particular enumeration than would be admissible here; but the total number of active ones known, amounts to nearly two hundred, of which one half belong to the American continent.

Of the general Characters and geological Connexions of Volcanoes.

The forms of volcanic mountains are peculiar. They are generally lofty, and, when solitary, of a conical form; being also more or less truncated at the summits. When active, or but recently extinguished, that truncation is accompanied by an irregular cavity, which forms the crater. In Vesuvius, and in other Italian volcanoes, the crater is a very remarkable feature. It is easy to understand the cause of these forms; the conoidal shape, like the crater, being the consequence of successive eruptions from the summit. Hence, also, volcanic mountains change their shapes; while the edges of the craters or the summits of the cones are often demolished, and again, sometimes replaced by new ones: and thus too, eruptions at the sides produce subsidiary mountains with craters of their own, which modify the form of the principal mass. These several events have occurred in Ætna, in Vesuvius, and in Vulcano. In America, they present a great diversity of forms. But the Moon will convey the most tangible idea of many of these varieties; since every thing is here displayed as in a model, and often in a very explicit manner.

The accurate form of a perfect crater is that of an inverted conoid; and, on Cotopaxi and Teneriffe, they are surrounded by walls of lava. The bottom of the crater is generally irregular; being sometimes a plain, of greater or less extent, containing small irregular cones, among which, during periods of comparative repose, various volatile matters are discharged. Thus the present cone of Vesuvius appears a new production, raised within a more antient crater, of which Somma is a remaining portion. The size of the Crater rarely bears any proportion to that of the mountain: in Vulcano it is very large, in Teneriffe small; as it is in many of the volcanic mountains of the Andes, although that of Cotopaxi is of great size. The depth varies after every eruption; and, in Vesuvius, these changes are frequent, though the crater of Teneriffe has for a long time appeared very constant. In some eruptions, a volcanic cone has suddenly disappeared, leaving a lake in its place;

the origin, it appears, of the lakes of Averno and Agnano. That the ground is cavernous beneath the craters of volcanoes, is proved by their sound, and by the vapours which they emit; appearances remarkable at Vulcano and in the Solfatara; the latter being a tract which has maintained its character unvaried since a period anterior to Roman history, giving rise to some of the well-known mythological fables of the antients.

In Europe and in Asia, those volcanoes which are well known, appear to be detached, whatever unknown communications they may possess beneath; but in America, they are sometimes arranged in rows, as in Chili and Guatimala. Antisan and Pichinca in Peru, are prolonged ridges. This fact has given rise to some speculations on the seat of the fire, which will come under review hereafter.

The peculiar position of marine volcanoes renders it necessary to record a few of the appearances which they present. In 1707, near St. Erini in the Greek Archipelago, a volcanic island was formed, which, in less than a year, attained a circumference of five miles, with an altitude of forty feet. A similar one of smaller dimensions was also produced in the same place. In the same manner, islands have been generated near Iceland and the Azores, at different periods; and it is not long since a small spot was thrown up among the latter; shortly after washed away, from having been formed of loose materials. In the Pacific ocean, these events have frequently happened; and though the islands thus produced are sometimes low, they occasionally attain a considerable altitude, as is the case with Ascension. But I may refer to the accurate work of Von Hoff for numerous details on this subject.

The geological connexions of volcanoes have not often been clearly ascertained, nor perhaps are they of much moment. If the seat of the fire is as deep as the phenomena would indicate, their appearance and place can have no necessary connexion with any of the rocks at the surface; since they must break out through whatever obstacles happen to oppose them. It is generally difficult to make the observations, because the surrounding rocks are, in most cases, covered by the produce of their eruptions, so as to be rendered invisible. Hence volcanic rocks alone are to be found in the neighbourhood. Those of Spanish America are often indeed situated among traps and porphyries; but there can be no doubt that these are volcanic products, ejected under a degree of pressure which has prevented the disengagement of the aëriform matters. If they could be proved to be trap rocks, formed therefore before the emergence of the present strata, or at that time, they would establish the identity between trap and volcanic products, and also prove that the seat, as well as the origin of both, was the same; thus explaining the tendency of the trap rocks to recur more than once in the same place, and further, the limited and local nature of these productions. As these porphyries are connected with gneiss and micaceous schist, their volcanic origin is the more probable; for, with occasional exceptions sufficiently intelligible, the trap rocks are in contact with secondary strata.

In Auvergne, the volcanic matter has issued through granite; whence the lavas of that country repose immediately on this rock. In the African islands, the volcanoes appear to have elevated themselves from beneath the secondary strata; carrying up with them the calcareous beds now found in those islands; and, in the Pacific, I have already shown that they have brought up from the deep, the much more recent coral rocks. Where the original rocks cannot actually be discovered in their places, they are sometimes inferred from observing the unmelted matters ejected during eruptions; and thus it is inferred, that granite, micaceous schist, and primary limestone, lie beneath the present materials of Vesuvius.

On the Action of Volcanoes.

The action of Volcanoes is generally periodical or intermitting. Yet Stromboli is not only now in a state of constant activity, but is known to have been so since the year 292 before the Christian æra. Jorullo has also continued to flame ever since it first broke out. Vesuvius has been known to remain inactive for centuries. Ætna and Teneriffe have also intermitted for a century together; and Vulcano was free of eruptions for eleven hundred years.

The appearances which attend eruptions are various, and, in some points, instructive; but as I can only give a meagre sketch, I must refer to the endless descriptions of authors. As the consequences are no less terrific than picturesque, they have not only led to highly coloured descriptions, but have misled and dazzled those whose minds were not steadily bent on realities.

The most simple and precise account is that given of Stromboli by Spallanzani. The crater of this volcano always contains melted lava, in constant motion. On a sudden it becomes raised to a higher level, and when it arrives near the edge of the basin, a sound resembling thunder is heard, and a portion of the lava is thrown up into the air, accompanied by dust and smoke. Large bubbles appear and burst at the moment of the explosion, after which the lava sinks again to its former place; these appearances being repeated incessantly at short intervals.

In the intermittent volcanoes, the phenomena vary, and are more or less violent. The eruptions of Vesuvius and Ætna have been most studied, and have been the subjects of numerous interesting narratives, from the time of Pliny to our own days. In general, the first appearance of an eruption consists in a column of smoke, rising to a great height in the atmosphere, and then spreading out in a form compared by Pliny to that of a pine tree. This is followed by explosions, by trembling of the earth, and often, by decided earthquakes. Flame is then seen to issue from the crater, attended by red hot stones, which are often thrown high into the air; producing those brilliant effects which have so often been described. Lastly, the melted lava, rising to the edge of the crater, flows over it, or, in some cases, breaks out at a fissure formed in the side of the mountain. These currents are often very extensive; reaching to many miles in length and breadth, and flowing on with a constantly diminishing rapidity, for many months; the issue of the lava from the opening continuing for an indefinite period. The eruption of the lava is often followed by small fragments of rock, commonly called ashes; being the same substances which constitute the black smoke, and which, in the shape of fine dust, are often carried by the winds to enormous distances. Globular masses of lava also fall with it; appearing to have been projected in a fluid state, and to have been consolidated in the atmosphere.

During the emission of the dark smoke, electrical matter is produced, appearing in vivid and frequent flashes of lightning, and adding much to the splendour of the scene. At the same time, sulphureous gas is generated by the combustion of sulphur; and, in some eruptions, white smoke, apparently the steam of water, is produced in considerable quantities.

The appearances now described are drawn chiefly from the eruptions of Vesuvius, of which more than thirty have been recorded since the reign of Titus. By these, Pompeii and Herculaneum were overwhelmed, preserving invaluable records of Roman times and manners; and, by one of them, Monte Nuovo, three miles in circumference, was thrown up in forty-eight hours. To convey a further notion of the quantity of matter ejected, Herculaneum is now seventy feet below the surface, and Pompeii ten or twelve.

Breislak has been at the trouble of computing the quantity of lava ejected by some remarkable eruptions in different places. That which flowed from Vesuvius in 1794 was estimated at nearly 3,000,000 of cubic fathoms. In 1796, the mass of ejected matters in Bourbon was computed at upwards of 9,000,000, in 1787, at nearly 12,000,000, and in 1791, at 8,000,000. One current of lava from Ætna is stated by Recupero to be forty miles in length; and that which flowed in 1669 measured fifteen miles by seven in breadth, while it filled all the intermediate valleys. One of the eruptions in Iceland is said to have covered a space of ninety-four miles by fifty, or four thousand seven hundred square miles. These facts will convey a notion of the immense masses of matter that are thus brought up from the subterranean regions which are the seat of the volcanic fire.

In many cases, as in those of Ætna and Teneriffe, the volcanoes smoke, without burning, for a long period: while others emit flames, without either smoke or lava. In some of the eruptions of Ætna, minute fragments and dust have been thrown out, while the mountain only smoked in its usual tranquil manner. Many of the volcanoes of Quito eject flames, water, and mud, without lava. Even fish, almost living, or recently dead, are sometimes thrown out with the water.

The eruption of a volcano for the first time, is attended with some difference in the phenomena, though these have hitherto been examined only in volcanic countries, That of Jorullo was preceded by earthquakes for a considerable period; after which a large tract of ground rose to a height of more than five hundred feet, forming a convex hill, whence issued flames extending over a wide space, together with fragments of heated rocks. This surface was in a state of undulation resembling that of the sea, and two rivers were swallowed up in the abysses which it formed. Torrents of mud with stones were then thrown out, and numerous cones arose in the neighbourhood; while one large chasm was formed, whence were projected masses so bulky as to form hills of sixteen hundred feet in height. Jorullo itself, still burning, is the highest of these.

In the history of this eruption we can trace the origin of those chains which were formed on the declivity of Ætna in 1809, and on that of Vesuvius in 1760. It is probable that similar phenomena accompanied others long since extinct; such as those of Auvergne, which are ranged in a line of sixty miles in length. Thus also, successive eruptions may at some future period raise Jorullo to the height of Vesuvius or Ætna. It is easy also to understand, how an eruption thus taking place for the first time in a new spot, may be attended with unusual violence; from the greater accumulation of the expansive matters caused by the superior resistance.

Eruptions of singular and extraordinary violence have occurred in Java. In one of these, the volcano Papandyang entirely disappeared, having fallen into the earth. The extent of the land thus swallowed, was estimated at fifteen miles in length by six in breadth; and that which remained was so far levelled as to stand but three feet higher than the surrounding plain. This accident was attended by an earthquake, and by an eruption of volcanic matters. In another, an eruption took place in Sumbawa, one of the Molucca islands, in 1815. The island of Java, three hundred miles distant, was darkened and covered with ashes; while the explosions were heard for many days, not only in this island, but in Sumatra, at a distance of seven hundred miles.

The phenomena which attend the eruptions of submarine volcanoes, are sometimes of a peculiar nature. The islands thus produced, are sometimes formed by the direct elevation of the submarine strata; as happens in the Coral islands. In these cases, eruptions of fire and smoke have been observed. In other instances, stones and scoria are thrown out from the volcanic aperture; which, gradually reaching the surface, become consolidated by their own weight, and by admixtures of lava. The pumice so often found floating in the sea, is probably often produced by these causes; having been too light to consolidate itself, and being thus washed away by the waves. Thus the small island of Sabrina, near the Azores, was entirely destroyed not long after its formation; having been formed entirely of light scoria. Such recent volcanic islands have been produced on the coast of Iceland.

Therasia, Automali, and Thia, according to Pliny, were thus generated near Santorini; and, more recently, some smaller ones have been formed in the same place, as before mentioned. In these, as in other cases, flames and smoke have appeared in the sea, marine earthquakes have been felt, and the water has been heated to boiling.

Of the Produce of Volcanoes.

The productions of Volcanoes are numerous and various; consisting of different gaseous matters, of inflammable substances, and of various salts, minerals, and rocks. A very brief notice of the greater number of these must suffice; as we are little able to throw light on their origin, and as the minerals, which are among the most interesting, belong to the department of the mineralogist.

The formation of flame is attributed, and apparently with reason, to hydrogen gas produced from the decomposition of water; and its evaporation appears to be the chief cause of the white smoke, resembling cotton, or the produce of the high-pressure Steam Engine, which is sometimes thrown up. It has also been ascertained that Vesuvius throws out carbonic acid, azote, muriatic acid, and sulphurous acid. Sulphur is said to be sometimes ejected by eruption; but it is, in general, sublimed by a slower process, during periods of comparative repose. Boracic acid is one of the rarer productions; occurring in Vulcano: and the formation of siliceous stalactites, apparently deposited from the steam of water, in Vesuvian eruptions, must also be considered rare. Tourfan, in Tartary, produces muriat of ammonia.

It was already remarked, that the black smoke of volcanoes consisted of dust, or of the minutest fragments of rocks; and this also appears to be the chief source of the mud which has often been supposed to flow out of the volcanic opening. During the eruption, torrents of rain fall on the mountain; and, mixing with the dust, produce those currents of mud which seem chiefly to have overwhelmed Pompeii. But, in other cases, as boiling water is also ejected, either from the water of rivers, or of lakes, or of the sea, getting access to the burning caverns, it is not improbable that this is also an occasional source of the eruptions of mud. That the fishes already mentioned, Pimelodes Cyclopum, should be ejected without destruction, proves, that the water had, in these cases, been thrown out almost at the moment of its admission to the sources of heat. The torrents of water which sometimes accompany eruptions, are also occasionally caused by the melting of the snows on the summits of the mountains; an event which has frequently occurred in the Andes, and been attended with great destruction.

But I must here also mention those singular productions, the mud volcanoes, which exist without apparent fire. One of these is found in Java, one in Sicily, and others in the Crimea. That of Maccaluba in Sicily, is a hill terminating in a plain of mud, which, when softened by the rains, is in a state of ebullition, and, when dry, is elevated, in various places, into little cones with craters, whence a black mud is emitted. In some seasons, the eruptions are sudden and violent, and attended by slight earthquakes. Fragments of rock are sometimes also ejected, together with sulphuretted hydrogen: and this general account is sufficiently applicable to all the appearances of the same nature that have been described.

But the most interesting products of volcanoes, to the geologist, are the rocks and minerals which they eject; as these throw light on many obscure and disputed points in the history of the unstratified rocks. I shall not enumerate the long list of volcanic minerals, as they are to be found in all the treatises on mineralogy. The only question of interest here, is the mode of their production; and I have sufficiently shown, that the greater number are the produce of fusion, though some are formed by watery infiltrations into the cavities of the scoria; a case identical with that of the generation of the amygdaloidal nodules in the trap rocks. If it is not always possible to distinguish the two cases, the following general rule may still be commonly applied to them. When the minerals form part of the solid rock, whether it be scoriform or not, and particularly when they impress and interfere with each other, they must be considered as the produce of fusion; where, on the contrary, they occupy the caverns of the scoria, whether filling them or not, they will generally be found to arise from posterior watery infiltration.

The unfused rocks ejected by volcanoes require no particular notice, as they may consist of any of the numerous substances that chance to lie in the way of the volcanic explosion· It is easy to understand how they may be thrown out little altered, as has sometimes happened to limestone containing shells; and how by falling back into the crater, and being then exploded again, they may bear, in a greater or less degree, the marks of heat, or even of superficial fusion. The dust and fragments may, in the same manner, be the produce of the natural rock, or of the antient solid lavas which form the crater and the internal cavities of the mountain. Hence the puzzolana and the tufas of volcanoes; and hence also, when mixed with water, the eruptions of tenacious mud.

The rocks ejected in a liquid state, are the most important of the volcanic products, and are commonly divided into obsidian, pumice, scoria or cavernous lava, and solid lava; presenting great varieties of aspect and composition. The distinctions between these are not however always very definite; nor, in a geological view, are they of much importance.

The consolidated currents of lava consist of a greater or less variety of these rocks, and form irregular beds, somewhat resembling strata when in particular positions. They are of various sizes, and often very extensive. As they are frequently repeated in the same spot, they are also found in irregular succession, with layers of dust or puzzolana sometimes interposed. In Iceland, they have occasionally been fused in their places without flowing. Those which have flowed under the sea, or under other more antient lavas, are often compact; while those which have run on the surface, are porous or cavernous; from that disengagement of air which, in the others, has been prevented by the superincumbent weight. In a few instances they form columns resembling those of the trap rocks; and they also present examples of that laminar concretionary structure so remarkable in trap, and which occurs in granite also.

The decomposition of lavas is very unequal in respect to the time which it occupies; and this arises from the very variable nature of their composition; while they are noted, on Ætna and Vesuvius, as well as elsewhere, for producing, like the traps, remarkably fertile soils. From false calculations respecting the time required for this decomposition, and from mistaking interposed tufas for the effect of this, there have been deduced groundless conclusions respecting the ages of eruptions, which it is now no longer necessary to refute.

It has been idly disputed whether lavas could ever be compact: and hence they who have denied this possibility, have referred all the compact volcanic rocks to trap. This has been a fertile source of errors which it is important to remove; both as it regards the history of the former and that of the latter substances. The question can never be decided, it is evident, if every compact lava is considered a trap rock; since this is to assume the point to be proved. The volcanic rocks of Auvergne are often compact, as are many deep currents of lava which have been examined in many parts of the world. And as, in many modern lavas, the size of the pores diminishes as we descend deeper in the bed, the possibility of this state of absolute solidity, and the nature of the cause by which the variable nature of lava in this respect is influenced are proved. It is elsewhere shown that the same variations exist in the trap rocks; and unquestionably from the same cause, or a greater or less degree of pressure acting on the fluid material. The analogy of lavas, in many particulars, to the rocks of this class, is striking; as must be the case, since their origin is analogous or identical. Much additional light would be thrown on the latter, from a more accurate history of volcanoes than we have yet had. It is an important point to ascertain, whether the rocks, apparently of the trap family, in which volcanoes, or lavas, are often situated, are the produce of their eruptions, and, further, whether they are the produce of eruptions within recent times, or during the present state of the globe. If such formations of trap can be certainly distinguished from the ascertained produce of recent volcanic eruptions, and if they resemble in every point that family, while they differ essentially from the present volcanic rocks, it might be concluded that such volcanoes, under some other condition possibly, were anterior to the present state of the globe and were permanent sources of subterranean heat. Thus also it might be inferred, that the present overlying rocks occupied the places of antient volcanoes, though differing perhaps in some essential circumstances from the existing examples.

It is not here necessary to describe minutely the various characters and composition of the lavas: all that may be required on this subject, will be found in the Classification of rocks. In a general sense, they present a much stronger resemblance to the traps than to any others, as well in their aspect as in their constituent minerals; though it is in this last circumstance that the most important differences will be found. Yet even these differences, when most striking, are rather of a local than a general nature: the leucites of Vesuvius are almost peculiar to that mountain, as are many of the very compound lavas in which it abounds. Augit, and compact felspar, or a mineral analogous to this last, form the leading ingredients of lavas, as they do of many of the trap rocks. With respect to general character, the lavas are simple and compact, or porous, or amygdaloidal, or porphyritic, or of a granitic character; and two or more of these features are sometimes united in one example. Where the occasional minerals are numerous, they sometimes form the great bulk of the compound, as is the case with Leucite. Obsidian is a glass; and pumice varies, only as it happens to consist of this glass inflated by air or steam, or of other lavas rendered so thoroughly cavernous by inflation as to float on water.

Of the Seat and of the Theory of Volcanoes,

As the disputed question respecting the seat of the volcanic fire, is intimately connected with the theory of Volcanoes, it is necessary to consider these two subjects together. It has been imagined that the seat of the volcanic fire was superficial, and that it lay among the coal strata; the eruptions having been attributed to the combustion of this substance. It is to trifle with a reader of any sense or knowledge, to bestow a serious thought on such puerilities. But the propounder was the greatest of Geologists; the condition of Geological science is not to be wondered at. Were such strata proved to exist beneath volcanic mountains, their depth would not be sufficient to produce effects which bespeak the profundity of the abysses whence such enormous masses of matter and such extensive disturbances proceed. Besides, in the antient volcanoes, such stores of coal, even had they existed, must long ago have been consumed; while the black smoke of volcanoes does not consist of inflammable matter, and they eject no bitumen. But it is useless to enumerate other objections of equal weight. Be the cause of the fire what it may, it is not seated within the volcanic mountain. Had that been the case, many of these would have been destroyed, instead of having constantly received, for such long periods, such vast accessions of matter. This alone proves that the cause is deep seated, and that the cavities which have supplied the volcanic portions of Ætna, Teneriffe, or Vesuvius, are so remote as to be protected from the great weight of these mountains by the crust of solid earth which lies above them. The phenomena of submarine volcanoes also prove the great depth of their causes; as, from no other sources could such masses of matter be elevated from the bottom of a deep sea.

The same depth is proved by the obvious connexion which subsists among the Italian volcanoes; and if, as formerly suggested, a large portion of that country has been raised from the bottom of the ocean, the source of expansion must lie very deep in the earth. The phenomena of earthquakes, the effects of which are often propagated to such enormous distances from their immediate seat, prove the same theory; as it will immediately be shown that they arise from volcanic actions.

It might be inferred from the antient volcanoes of Auvergne, that the place of the heat was one prolonged chasm, the fires of which, issuing from a great depth, have found a vent in different places; and this opinion is strongly confirmed by the American volcanoes, particularly by the circumstances with which the eruption of Jorullo was attended. There is a narrow belt, lying between the parallels of eighteen and twenty-two degrees, in New Spain,in which many burning and extinct volcanoes are situated. That a continued fire exists here, is proved by the presence of boiling springs, by the occasional eruption of smoke, by frequent noises or explosions, and by earthquakes. In this line are the volcanoes of the Pic d'Orizaba, la Puebla, Nevado de Toluca, and Colima; that of Jorullo lying in the same parallel, and nearly at right angles to the Cordillera of Anahuac. This line, extending for one hundred and thirty leagues and upwards, seems to indicate the existence of a volcanic chasm, which may probably extend even to the Archipelago of Revellegedo, and may also be the common deep seat of the whole. Hence the great depth of the volcanic regions is here indicated by a species of evidence which appears uncontrovertible. It is equally probable that, like the American and Italian volcanoes, those of the Canary isles are merely the separate vents of one volcanic abyss, which must there also be very deeply situated. I may here also remark that the volcanic region of Sardinia extends in a line through the whole length of that island.

The causes of the volcanic fire have, naturally, been subjects of much speculation. The theory which attributes this to coal has already been examined. The ignition of pyrites has also been a favourite theory with those who saw nature only in the minutiæ of their laboratories and cabinets. There is no such pyrites present, the products of volcanoes are not those which would result from such a cause, and it is neither able to explain their duration, their intermission, nor their deep and distant connexions. The same answer nearly may be given to the theory which supposes sulphur to be the cause; and that which attributes it to bitumen is equally unworthy of further notice.

As, in justification of these theories, it has been asserted that the temperature of volcanoes was low, and that the rocks were not melted by the ordinary process of fusion, I must bestow a few words on a speculation which derives no merit but from its extraordinary absurdity. That the bitumen of Kirwan or the sulphur of Ferrara, should produce the fluidity of lavas without fusion, are propositions which do not deserve a serious examination; even could it be proved that either of these substances were present in lavas; which they are not. The project of Dolomieu for causing the parts to slide on each other, must be passed over, in compassion to a well-meaning observer. They who could adopt it from him, under the lowest knowledge of chemistry, deserve none. These theories have been contrived because it was supposed that certain fusible minerals contained in Volcanic products, were crystals that had been ejected in that state; whereas they are the produce of igneous fusion and slow cooling, of affinities exerted in a compound fluid mass of earths. Thus does ignorance generate folly. That the phenomena of Volcanoes all indicate an intense heat, would be as superfluous to prove, as it would be to show that those lavas which once formed uniform fluids resembling glass, are now crystallized rocks of an infinite diversity of character. Of the theories of Thomson and Patrin, one of them is impossible and the other unintelligible; but there are two other hypotheses which demand more notice, as the last also is probably the true one.

Since it has been known that the earths and alkalies were the oxydes of metallic substances, it has been supposed that the admission of water to repositories containing their metallic bases, was adequate to the solution of volcanic phenomena. This hypothesis has a merit which must be refused to all the others, because its chemistry is true. But it does not provide for the perpetual heat maintained in volcanic regions, nor for the long intervals of repose, nor for the production of new volcanoes. Water must be admitted in stated modes to produce these effects; and there is no agent to provide for its admission, or to open the fissures through which it must flow upon these inflammable substances. But a more serious objection arises from considering the nature and abundance of the unstratified rocks, of which the origin is similar to that of volcanic substances. It is plain, that to form all the granite and trap of the earth, an enormous quantity of water must have been consumed, attended by a proportionate disengagement of hydrogen, which is not now in existence.

The last hypothesis is that which supposes the perpetual existence of a central fire, or heat, ready to produce inflammation and its consequences, whether from the admission of water, or from internal chemical actions occurring in consequence of changes respecting which we cannot, at present, easily speculate. As this doctrine forms also the basis of the hypothesis which relates to the unstratified rocks, it connects, by one common cause, those things which are so exactly connected in their natures and appearances. The permanence of this central heat is indicated by the permanence of volcanic fires, and by the phenomena of hot springs. The force exerted may be the consequence of the changes already mentioned, or of an expansion, the nature of which has not been explained. It is plain, that as far as the excitement of the action by water is concerned, it does not labour under the same difficulties as the last; as it is not necessary that the water should be decomposed, and as the supposed expansion might produce, at irregular intervals, those fissures which are necessary for its transmission. The objections which have been made to this theory are of no force, as far as relates to its power, its properties, or its modifications; and by adding some not unreasonable assumptions, it is easy to modify it to all the requisite ends. It is but one portion of that general theory which includes the causes of the figure of the earth and of all its important revolutions; of which also, the phenomena of volcanoes form one of the leading evidences.

It assuredly presents a more magnificent and consistent view of nature, to consider Volcanoes as parts of one great system, and not as casual and partial phenomena, whatever theory we may adopt for their explanation. We cannot but suppose that operations so important are all dedicated to one great end. And the views of the origin of the unstratified rocks given in this work, being just, it is easy to see what that office is, of which Volcanoes form a part, or of which, at least, they are the proofs and living records. Successive eruptions of granite and antient porphyry, the same successions in Trap rocks, the elevations of Italy, those of the Coral islands, Ætna and Jorullo, this is the chain, and it is entire.

The resemblance between many of the trap rocks and the volcanic productions, has been here established; and these are strongest when we compare the most antient of the volcanic rocks with the former. A very perfect resemblance in all the essential circumstances has been also shown to hold between the trap rocks and those of the granite family. If all this be conceded, the whole of these are parts of one great Volcanic system, operating at different periods and under different circumstances, from the very commencement of the earth to our own days. Under such variations, modern volcanoes may be destined to form future trap rocks: even now they are probably forming beneath the sea, and, possibly also, beneath the land, productions of the same nature, as they are also producing islands above it. Our own traps, and even our granites, must then also be the produce of antient volcanic fires; differing in numerous circumstances which it cannot now be necessary to repeat. Many of these effects may have been purely submarine, lifting with them the strata which now form the earth's surface. Where they have broken through the strata, as they have so often done, it is easy to understand how, in the lapse of time, all the peculiar appearances which distinguish modern volcanoes may have been wasted away; how the craters, the narrow and shallow currents, and the loose materials, may have vanished; leaving nothing remaining but those solid rocks which are no longer distinguishable from trap; or rather which are real members of that family.

If it is still a doubt among geologists whether the Euganean hills are formed of trap, or whether they are of volcanic origin, we must expect similar difficulties elsewhere; and till geologists shall think fit to adopt more philosophical views, such difficulties must ever remain; as they will be always explained by the particular prejudices of the observer. The very volcanoes of Auvergne might thus also equally claim to be of the trap family, as they were once supposed: while they form that intermediate link which is wanting to connect together two very distant periods of eruption, or the two classes of phenomena, and thus to prove the identity of both. Hence the Volcanic Theory of the Earth, as it has been called, assumes a form, which those who have taken a different view of the effects of internal heat had neither the knowledge nor the judgment to understand. The theories are the same, under different names: but the self-imagined improvers on Lazzaro Moro constructed a system out of their own ignorance, disclaiming the teacher whose facts they could not comprehend; and willing, too, perhaps, to pass for inventors. The volcanoes which now elevate great tracts of land, whether from the bottom of the sea or on the shore, have operated the same effects in more antient times. I have shown that these have been partial as well as general, and that they have also been of different ages. By attributing the greatest catastrophes of this nature to the same causes as the smallest, an igneous system becomes rational and uniform; as the present views give the strongest support to that one which has, very ignorantly, thought proper to disclaim it.

Of Earthquakes.

It yet remains to examine the phenomena of Earthquakes; a subject so intimately connected with the present, in all its circumstances, as to leave no doubt of their being effects of a common cause. Theories of Earthquakes are as antient as the times of Anaxagoras, and are as numerous as the singularity and importance of the phenomena might lead us to expect. It is unnecessary to revive these hypotheses; and, with respect to the only one requiring notice, the electrical theory, it is sufficient to say that it is as gratuitous as it is irreconcileable to the appearances.

History has recorded Earthquakes from all times; the disappearance of mountains and islands, new lands produced, lakes and rivers lost, cities overwhelmed, and even their vestiges destroyed. Of the most noted in antient history, is that which swallowed up twelve cities of Asia Minor in one night, in the reign of Tiberius, with those which took place in the reigns of Gallienus, of Valens and Valentinian, and of Justinian. A remarkable one occurred in modern times on the coast of Puzzuoli in 1538, which annihilated the lake Lucrinus, destroyed a town, and raised the present Monte Nuovo to the height of eleven hundred and twenty-seven feet. In 1638, the earthquake of Calabria, described by Kircher, swallowed up the town Euphemia; and, in 1692, Jamaica was the seat of one which destroyed Port Royal, sinking the houses thirty or forty fathoms deep. At this time also, nearly all the houses in the island were thrown down, and many ships in the harbour were forced on shore. The great earthquake of Sicily, in 1693, destroyed fifty-four towns; but those of Italy are innumerable.

The earthquake of Lisbon, in 1755, was one of the most extensive in its effects of any that has been described, and is, on that account, particularly interesting. It was felt all over Portugal and Spain, in Madeira, and in some parts of Africa, and, in a slighter degree, in Sweden, Italy, France, and England. The waters of wells and lakes showed the disturbance of the earth, in many places where the tremor was not remarked; becoming agitated, turning muddy, overflowing their banks, or fluctuating and rising in pyramidal waves. Even the sea was thus affected; not only on the shores of these countries, but far off in the ocean. The places where these effects were perceived, were Norway, Sweden, Germany, Corsica, Switzerland, Antigua, Barbadoes, and Ireland; marking the deep seat, as well as the wide range of the disturbance. The last events of this nature which are here worth recording are those continued earthquakes which took place in Calabria between 1783 and 1786; involving a circle of seventy-two miles in radius, and producing the destruction of twenty thousand persons. In these catastrophes, a hundred and eighty-two towns and villages were demolished, and ninety-two injured; and the circle of complete destruction, of which Oppido was the centre, had a radius of twenty-two miles.

Earthquakes are most frequent and complete in volcanic countries, and are expected when volcanoes have been long dormant; a proof of the community of their causes. They are generally preceded by various unusual meteoric phenomena, by a peculiar state of the atmosphere, attended with uncommon sounds resembling thunder. Wells and springs are rendered muddy, or dry up, or become more abundant;

and the sea swells and roars, generally undulating in an unusual manner, even without wind. During the commotion, the shocks are generally numerous, succeeding each other with various degrees of rapidity. The earth undulates, heaving and alternately subsiding; or, in slighter cases, is merely in a tremulous state. In violent earthquakes, it opens; and, in these fissures, which are often of great size, towns and animals are ingulfed. The fissures sometimes emit smoke and flame, sometimes water; and flame and smoke are also often seen to issue out of the earth, without visible openings, as they do sometimes out of the sea also. The effect of the shocks on the water is remarkable; ships sometimes feeling the same sensation as if they had struck the ground, or received the blow of a wave.

The proofs of the connexion between earthquakes and volcanoes are innumerable; and the latter indeed often appear to give vent to that elastic matter which, being pent up, is the cause of the motion and the tremors of the earth. An earthquake extending to a distance of fifty miles, accompanied the eruption of flames in the sea of Azof which attended the formation of an island; and the ejection of Sabrina off the Azores, was also marked by the same circumstance. The same occurred when the volcanic islands arose out of the sea on the coast of Iceland. The great earthquakes of Sicily and Calabria were accompanied by eruptions of the Lipari Isles; and those of Ætna, Vesuvius, and other volcanoes, have equally been attended by earthquakes. Those of Cumana were connected with similar phenomena in the West Indies; and are supposed to have been dependent on the eruptions of the Andes. The earthquake of Quito in 1797 was marked by an eruption in Guadaloupe; and the volcano of St. Vincent broke out during the great earthquake that destroyed Caraccas in 1812. Innumerable other instances of the same connexions might be produced; but I shall only add to these, that, on the night in which Lima and Callao were destroyed, four new volcanoes broke out in the Andes. The great depth at which the causes of earthquakes are situated, is proved by all these phenomena, as well as by the great distance to which the effects of the Lisbon earthquake, and of others, extended. The earthquake of Lima was felt at a distance of four hundred and fifty leagues at sea.

These facts comprise all which it is necessary to say on the Theory of Earthquakes; since it thus rests on the same foundation as that of volcanoes, whatever this may be. Yet some phenomena of this nature which have occurred, are still difficult of explanation; either from their peculiar nature, or from their being unattended by eruptions in any part of the world. The late earthquake in the East Indies overturned two spots of ground at no great distance, leaving an interval of absolute rest between them. Small shocks of earthquakes are also not unfrequent in many places, even in Britain; and Loch Earn in Scotland has, for many years past, been noted for the frequent recurrence of these commotions. Whatever their differences may arise from, or wherever these effects may take place, it is probable that the same general cause applies to all, and that this solid earth on which we tread is only the surface of an abyss, in which extensive operations, unknown to us but by these their effects, are in a state of constant action; ready at any time to produce the most tremendous revolutions, from accessary causes with the nature of which we are equally unacquainted.

CHAP. XIX.

On Mineral Veins.

In a practical view, this is one of the most important subjects in geology, yet, though long studied by miners and geologists both, nothing is less understood. No general practical rules have been established, as to the districts or rocks in which they should occur, the courses they may hold, the forms and accidents they may display, or the substances they may contain. And as little have we been able to form any theory respecting their origin and the mode of their production.

Mineral veins may exist without metallic substances, and many minerals are occasionally found in repositories which cannot be called veins. Metallic minerals thus occur in the compound rocks, so as to form parts of their composition. Thus oxydulous iron is found in granite, gneiss, sandstone, and trap, molybdena in gneiss, and iron pyrites in micaceous schist, slate, shale, and limestone. They sometimes also occur independently; neither forming part of the composition of rocks, nor included in distinct repositories. In this way, pyrites is found in many situations, copper in the trap rocks, and oxydulous iron in volcanic ones. Some of these, also, are occasionally accumulated in such quantities in particular spots as to be wrought for use; and thus Cobalt and Copper occur in sandstone. Iron, in the form of iron stone and bog ore, forms beds; the first among the coal strata, and the latter in alluvial soils. Thus also, tin and gold are found among these, and the latter in great abundance; but, in the two last cases, the origin of the metals is in distant veins. Manganese also occurs in the form of beds, as has been said to happen with repect to mercury, copper, lead, and silver; but it must be remembered that veins holding a course parallel to the including strata, have sometimes been mistaken for beds. I suppress the provincial terms, flat veins and rake veins, as I have avoided those on all other occasions, since no useful information is communicated by this practice, which also incumbers geology still further with terms, and corrupts the English tongue. To shroud in the mystic phraseology of an art, that which can be expressed in ordinary language, is the result of a poor ambition, or a proof of the superiority of the memory to the understanding.

Of the Forms, Positions, and Relations of Mineral Veins.

Mineral, like rock veins, intersect the strata at all angles, and are also occasionally parallel to them throughout more or less of their courses. They imply a discontinuity of the rocks through which they pass, being composed of matter which has entered into the fissures formerly described in treating of the positions of strata; and hence they are occasionally accompanied by dislocations of the including strata. As they may hold any direction with regard to those, so they may be placed in any position towards the horizon; though, from a mere comparison of chances, it is plain that they must be far more frequently inclined than vertical. Hence miners distinguish between the upper and under sides of a vein. When mineral veins occur in considerable numbers in any tract of country, they maintain a sort of general parallelism; as if all the fissures to which they owe their origin had been formed at the same time by some common cause, or had been produced by the repetition of similar actions. This also is sometimes the case where more than one set of veins exists, and where the posteriority of the one class is proved by their intersecting the other, as in Cornwall, where the more antient veins are directed, in a general sense, from east to west, and the more recent from north to south.

Their longitudinal extent must evidently be limited, but is often considerable. They have been traced for two, and even three miles, in Cornwall; and it is said that one vein in South America has been ascertained to extend for eighty miles. But such observations as this excite distrust, when we advert to the comparative length and breadth of such a supposed continuous fissure, and to all the circumstances under which these must have been formed. Inaccuracy and hypothesis unite to produce such assertions.

The breadth of veins is extremely uncertain, varying from less than an inch to many yards. It is believed, by some, that their depth is indefinite: it is at least said that this has never been reached by miners. If that were even true, it would not prove the truth of an opinion so improbable, when we consider the circumstances under which fissures must have been formed. When the separated or dislocated strata preserve an accurate parallelism, the same relative disposition must exist between the opposite sides of the vein; and we might thus imagine it interminable. But if these have lost their parallelism after separation, it is evident that under one modification of this, they must come into contact in some part of the series, and that the vein will therefore disappear. This reasoning takes only a simple view of the consequences resulting from the appearances; but if it should be admitted that the materials of veins were ejected from the depths of the earth, then indeed they may be indefinite, or at least, incapable of definition, in their downward progress. But this is speculative matter.

The absolute antiquity of veins cannot be conjectured; but there are two modes of judging of their ages, within certain limits. It is evident that they are all posterior to the induration of the strata, as they imply fracture of these: and should veins be found in the primary strata, not also existing in the secondary, they must be of an earlier origin than the deposition of the latter. The veins of Cornwall are, probably, anterior to the formation of the English secondary rocks, because they do not occur in the secondary districts; but to render the proof of this complete, we ought to produce secondary strata unbroken above these veins, or discover them after removing these. Thus also, if the lead veins of Derbyshire are really confined to the secondary strata, they are of more recent date than the last changes in the primary rocks which caused the fissures. A difference of age in veins is, however, proved where two co-exist, and where the one intersects the other. This circumstance is not uncommon on an extensive scale. In Cornwall, most, if not all of the easterly veins are intersected by the northerly; the former being metalliferous, and the latter wanting in metals. These intersections are attended by circumstances as interesting to geology as they are important in the art of mining, in which they are often the source of much labour and expense.

As the first class of veins is frequently attended by dislocations of strata, so is the second; and, with the consequence of dislocating the former set. Thus the effect of a second vein is to produce a shift in the first, often attended by peculiar circumstances in the state and nature of its contents. The extent of such dislocations in veins is variable; as might be inferred from the motions of the disrupted strata, in which they necessarily partake. These form an object of the highest interest to the miner; and it is through previous judgments respecting them, that he is taught where to seek for the continuation of that which he has lost. Experience in a given country often forms a guide for these; but rules so deduced cannot be extended to other countries, or to remote places. To determine whether the motion of one part of an inclined vein is to be termed an elevation or a depression, it is necessary to take the point of departure from the surface, as in the case of dislocated strata. When a vertical vein is shifted, it is evident that the adjacent rocks must all have been moved, by the same quantity, in a horizontal direction; an event, as formerly remarked, not favourable to the theory which supposes the fractures of strata to be the effect of subsidence. The last circumstance relating to the forms of veins, is their ramification. They are occasionally separated, and again reunited; certain technical terms being, in mining countries, applied to the intermediate mass. In other cases, they send out slender ramifications; and sometimes they are found to ramify at once into many small branches.

I have separated from that which is matter of justifiable inference respecting the ages of veins, what can only be considered as an hypothesis, neither intelligible nor useful. It has been said, by that school which has so long obstructed the progress of Geology, that there are epochas in metallic veins, or that the metals themselves are of different ages. Thus it is said that Tin is among the oldest metals because it is found in granite, and that lead is among the newest because it occurs in secondary limestone. I need not enumerate all the particulars contained in this unfounded assertion; while a few simple facts will annihilate the whole system. Cobalt occurs in granite, in many of the primary schists, and in the secondary sandstones. Copper has been found throughout the whole system, from granite up to Trap, inclusive. Lead is found alike in the primary and secondary strata, and Iron is universal. This short list of exceptions already overwhelms the rule. If, again, the nature, or imagined age of the rock which is traversed by a vein is to be made the criterion of the age of the latter, or of the included minerals, it must be remembered that a vein must traverse every rock that was in existence at the time of its formation. The vein that intersects the granite, intersects the superincumbent strata also; and tin, copper, or lead, as it may happen, will occur in every part of it. It may have required uncounted centuries to form all the strata, but the vein is, comparatively, the work of a moment. To attempt to classify metallic veins according to certain imaginary dates of formation, is to make systems which, except at Freyberg, philosophy disclaims. If there were a hundred, instead of ten or sixteen "*lead-glance formations*," we must be content to remain ignorant of the ages of all that we cannot prove by the uncontrovertible marks already indicated.

There is not one circumstance in the history of veins, whether we regard their forms, positions, seats, origins, or the nature and disposition of the minerals they contain, which can entitle us to conclude that they possess a resemblance or analogy throughout the world, that they are of definite and definable ages, or that they are in any sense of the word, general or universal. Yet this doctrine is supported by geologists who imagine that the mines of New Spain are similar to those of Hungary and Saxony. That Patrin, who imagined the earth organized and endowed with a vital principle, should protract a zone of copper, silver, and lead, from England through Europe, Asia, and America, is consistent.

Of the Seats and the Contents of Mineral Veins.

The nature of the rocks in which mineral veins are found, is an obvious object of inquiry, but it cannot be converted to any useful purposes. They may be said rather to belong to countries than to rocks; since, in one, that substance may be highly productive of veins and metals, which, in another, is deficient and barren. They are, however, most abundant in the primary or antient rocks, and are also more common in gneiss, micaceous schist, and argillaceous schist, than in granite or in the older porphyries. In the secondary, or recent strata, they occur chiefly in the lowest, as in the mountain limestone, and are scarcely found in the upper strata, or above coal. In the same manner they are rare in the later trap rocks; but if Hacquet's observations are correct, they occur at Nagyag, either in these, or in antient volcanic rocks.

In the primary rocks, they are sometimes found at the junctions of granite with the strata, as happens in Cornwall and at Strontian. But no practical advantages accrue from any thing yet known on this subject; unless under experience acquired in particular districts. The limitation of Tin to Cornwall and a few other spots, and its exclusion from countries formed of the same materials, the barrenness of gneiss in Scotland, compared with its fertility in Saxony, may be added to a thousand other instances, to prove that we must be content to possess mines wherever they are found, without wasting our means where we have no evidences of their existence. That much false philosophy should have been adopted on the subject of mines, is a natural consequence of that perversion of judgment which so often attends the pursuit of wealth, and is produced by examples of its sudden acquisition.

The contents of mineral veins are various; and though the metals form the most valuable of those, they bear a very small proportion to the rest. No general rules respecting these contents can be given, as they vary in almost every country, in every vein, and often, in every part of a vein. It is common, however, to find that the sides next to the including rocks are formed of earthy matters; sometimes of clay, at others of quartz, and, not unfrequently, of fragments of the including rocks, united by crystalline and earthy substances; while the rock is generally decomposed and altered at its junction with the vein, as detached fragments of it are sometimes also included within that. This occurrence has sometimes presented an interesting variation, where a vein traversing schist and granite together, has contained fragments of the former within the space bounded by the latter, and the reverse. This fact seems to prove revolutions of a mechanical nature in the vein, either at the time, or after the period, of its formation.

I need not enumerate all the earthy minerals which have been found in veins, but the most common are quartz, calcareous spar, barytes and fluor. These, like the metallic substances, occur in different parts of the vein, and are crystallized wherever cavities are present. The metallic minerals are variously disposed; sometimes lining similar cavities, in their crystalline forms, at others, collected into deposits in different parts of the vein; and, at others again, more generally diffused among the mass of materials. In some instances, only one metal is found in a vein, in others, two or more; and these are sometimes distinctly separated, at others intimately mixed, so as to be a source of much trouble to the miner. Occasionally, the minerals, whether metallic or earthy, are arranged in layers parallel to the sides of the vein; and, in some of these instances, there is further, a perfect correspondence on the opposite sides. Such also is the capricious disposition of the metals, that they sometimes disappear altogether, after having abounded through a large space; so that it becomes necessary to abandon a mine which had once proved profitable. It is from these perpetual variations in the contents of mineral veins, that the characters of particular mines are subject to such important alterations, and that chance, in the ordinary acceptation of the term, baffles all the calculations of the miner. Yet rules are still to be found in every mining country, of occasional value in practice, but always local; while they offer no facts on which a philosophical geologist can safely reason.

The intersections of veins sometimes produce variations in the nature and disposition of their metallic contents; but these, like most other rules, are of a local nature. It is also said that masses of ore are found at the intersections of more recent veins, and that intersecting veins of different periods, necessarily differ in the nature of the metals which they afford; that under peculiar modes of crossing, they become more productive, under others less, and that, after the intersection of a more recent vein, the metallic produce of the antient one disappears. The value of such remarks is not very intelligible, and the same proposition is often both true and false at the same time. Like many other conclusions of a similar nature, their chief value consists in warning us not to rely on observations guided by no principles.

There is one observation, however, respecting the variation of the contents of metalliferous veins, which is of importance towards a rational theory of them; if indeed it should prove to be really founded on facts sufficiently extensive. It is said that in all countries where veins traverse strata of different natures, their metallic contents vary with some relation to these; and that, in the same vein, the vicinity of some strata renders the vein more productive than that of others. But the facts are neither very numerous nor very definite: it remains to be proved whether they are not swallowed up by a mass of exceptions. It is said, that in a vein in Cornwall, passing through schist and granite, the copper is found in the former and the tin in the latter portion; that similar veins are poor in the schist and rich in the granite; that veins are most productive at the junction of the schist and granite, not only in that district but in Silesia and elsewhere. There is not one example of this nature to which there are not exceptions many times exceeding them, for which the reports of the same observers may be consulted.

But it is fruitless to record all the observations which have been made on these subjects; since the conclusion would be to draw, as might equally be done without them, no conclusions. Whether, as to the influence of strata over the contents of veins, any exception ought to be made in favour of Derbyshire, it is fruitles to ask, till miners have learned to observe more accurately, and geologists, discarding their systems, shall seriously turn their attention to a branch of the science which is most particularly its opprobrium.

Of the Theory of Mineral Veins.

On such a foundation it has been attempted to build theories of mineral veins; and, as usual in similar cases where the love of truth has no weight against temper and vanity, the opposing opinions have been maintained with a vehemence proportioned to the want of evidence. Philosophy would shrink into a small bulk indeed, were Truth its object. In stating these hypotheses for the purpose of inquiring into the probability of either, I must premise that the only important question at issue, concerns the manner in which the contents of the veins were formed and introduced.

It is said, on one hand, that the materials of veins have been deposited from the same universal solution whence the rocks were formed. But there are two modifications of this aqueous theory. While the rocks were in the act of being precipitated, the veins were undergoing the same process; and hence they are of different ages, corresponding to those of the strata in which they lie. How such an operation was effected is not explained; but such was of the worshipped geology and chemistry of that Germany which founded geological science; founding it on as solid a basis in all else. In the other modification, the fissures were formed in the rocks by drying, and the minerals were precipitated in them after the deposition of the rocky materials. He who can measure the relative impossibility of two impossibles, may take his choice.

When geology shall have forgotten all that Freyberg taught, it will have a clear field.

But if there can be one man requiring another answer, the precipitation of rocks from solution in water is at variance with the laws of Chemistry, and the objection would still be fatal, though the rocks had been produced in some other manner, and the production of veins alone was thus to be explained. Even if the power of this imagined solvent were granted as to their materials, it must be proved why the minerals of veins were not deposited every where alike, why they were not deposited in strata, why they were directed exclusively to fissures, why to a few of these in distant and select places, and why limited also to partial spots in the same vein.

If these objections are unanswerable, the few arguments from facts adduced in support of it, will require very little discussion. If many of the substances found in veins are the produce of watery solution, there are many others which, as far as we yet know, cannot be produced in this manner, as I shall hereafter show. It has been argued that the minerals of veins are deposited in layers parallel to their sides, precisely as ought to have happened on this hypothesis. In the first place, the fact is not so, except occasionally; as they are frequently congregated in irregular lumps, or dispersed among the other materials, or wanting for considerable spaces, or found lining the insides of cavities. Neither of these things should occur, according to the hypothesis; and especially, there could be no cavities on such a system of deposition from above, and the layers of minerals ought rather to be parallel to the horizon than to the walls of the vein. The argument derived from the presence of rounded materials in veins is worthless, because the fact itself is extremely rare. It is an exception instead of a rule, and may be admitted without involving the whole hypothesis.

Since there must be an igneous theory to oppose an aqueous one, in every thing, the other hypothesis maintains that the contents of mineral veins were injected from below as granite and trap have been. The arguments for it rest partly on this very analogy, partly on real or imaginary chemical facts relating to the production of minerals by fusion, partly on some mechanical appearances, and partly on the principle of dilemma. If it be really a case of dilemma, the one horn appears as fatal as the other, and there can be no theory of mineral veins.

The argument from the analogy of trap and granite veins is one of those superficial resemblances, consisting in words rather than ideas, which it is painful to find in the writings of those who have been philosophers in other things. It may be conceded that the fissures have been produced by the same subterranean changes which have displaced the strata; yet this admission does not involve a concession as to the rest of the hypothesis. The presence of fragments of the including rocks in the veins, which has also been used as an argument, is a fact of no further value: it proves the forcible displacement and fracture of the strata, but nothing more. And this hypothesis has forgotten, that, if the contents of these veins had been injected in a state of fusion, the fragments so often found in them should not have escaped this process. I will not say, as has been objected, that clay could not have existed in them on this principle; because the infiltration of water may decompose portions of the veins, just as deep seated rocks are converted into clay. As to the chemical arguments derived from the insolubility of many of the contents of mineral veins in water, and their production from fusion, they are founded on ignorance of Chemistry; as it is easy to show that many of them are certainly produced from solution, that others may have been generated in this way, and that some of them could not have been consolidated from fusion. I shall reserve these particulars for the end of this chapter, when the several minerals producible in either mode will be enumerated.

Whatever objections may be made to the aqueous hypothesis, from the dispositions of the minerals in the veins, they are at least equally valid against the igneous one. It is impossible to comprehend how these several peculiarities could have been produced from a state of igneous fluidity, any more than from a state of solution. It has also been said by the supporters of this hypothesis, of whom the ostensible one is well known, that the absence of the solvent from the veins is a proof that their contents were not deposited from water. It has commonly been supposed a necessary preliminary to correct reasoning, to possess knowledge. Geology has seemed to be exclusively privileged to dispense with it, and thence has it been what it is. Calcareous stalactites, travertinos, veins, quartz veins, chalcedonies, amygdaloidal nodules, are deposits from solution, and the water is equally absent. If this is meant to be an argument from dilemma, the first step is to establish the necessity of the alternative.

Another groundless chemical argument has been derived from the mutual impression of co-existent crystals in the veins. This is founded on the nature of granite and other rocks, crystallized from fusion; but it is the misapplication of a fact, through similar ignorance, equally evinced in attempting to explain the nodules of the amygdaloids. I have proved elsewhere, that the mutual impression of quartz, chalcedony, and calcareous spar, occurs in these, from successive infiltration and crystallization; and, according to the order in which these substances are deposited, either may impress the other. Thus might any number of minerals, admitted at distinct intervals into cavities, present the same appearances; and even in modes much more complicated than from any simultaneous crystallization in an uniform fluid of fusion. And, in reality, though the inconceivable chemical agencies required to separate all the minerals of a compound rock from solution in water, have been made almost a subject of ridicule against the supporters of aqueous theories of rocks, that is much better deserved by those who would crystallize all the variety of earthy and metallic minerals found together in veins, from an uniform fluid of fusion, as the chemist who is acquainted with a mineral vein well knows. It is hard that even the chemistry of Geology, where the better portion of Geology is Chemistry, should have been settled by those who knew nothing of that science. But such has been the fate of this unlucky branch of Natural history, in every thing.

Some further arguments, as much mechanical as chemical, have also been adduced in favour of the igneous hypothesis. It has been said, as an argument from dilemma, that on the aqueous theory, no close veins, or deposits of minerals surrounded on all sides by rock, could exist. But it is obvious that these are equally impossible on the other view of a cause. Where there is no access for a watery solution there is none for an igneous fluid. The inventors of "igneous secretion," applied here, as to the nodules of trap, ought really to explain a new process in chemistry; they have been fortunate that their opponents did not possess chemistry enough to retort the ridicule which they have not themselves spared. If mineral veins have, in any case, been filled by secretion from the including rocks, there can be no choice between a process which is proved to exist, and one which is unintelligible.

It has also been said that the solidity, or fulness, of mineral veins could have happened only from igneous injection; as the abstraction of the water after deposition, must have left cavities or vacuities of some kind. With no small want of reflection, it has further been asserted, that cavities could have been formed in them only on the igneous hypothesis, from the disengagement of elastic fluids. These, it is plain, are the conflicting statements of forgetfulness. The fact, such as it is, is quite as explicable on the one hypothesis as on the other, and is alike worthless to both; while the want of marks of gradual and regular deposition, is a negative which, if it proves one hypothesis to be wrong, does not render the other right. Such are the objections to an hypothesis which, however it might be deemed a necessary part of the general theory to which it belongs, does not involve the igneous origin of granite and trap, nor the elevation of the strata through heat. It has been the error of this, as of all other hypotheses, on all subjects, and at all times, to force all things into conformity to itself, without regard to facts, and without considering what was to be the gain. I should have considered this discussion as a mere waste of words had it not been for the strength of assertion which has been brought into this question on opposite sides; and if it proves nothing, it must be recollected that to show the existence of falsehood in these cases, is the first step towards truth.

Of the Minerals which are, respectively, produced from Solution and from the Action of Fire.

It remains to examine by chemical and mineralogical experience, how far any of the substances found in mineral veins are the produce of crystallization from watery solutions, and in what cases they are crystallized from a state of igneous fluidity, or from sublimation. If our information is still incomplete, a general view will be sufficient for the present purpose. The facts themselves, as they regard the two theories which have been examined, are singularly conflicting; though as far as they offer arguments for either, the balance is palpably in favour of an aqueous one, under some form, but not that of Freyberg. It is evident that these are the facts on which any future hypothesis must chiefly rest; whatever further considerations may be required for explaining the various other circumstances which attend mineral veins.

In inquiring, first, respecting the earthy minerals, so as to determine those which may be produced from watery solution, I must have recourse almost entirely to the chemistry of nature; as the limited solubility of the earths prevents us from deriving much information from our own circumscribed experiments. For the sake of brevity, I have thrown them into the form of a list; and, to save repetitions of the proofs on which their aqueous origin rests, these may be given in a preliminary form.

The formation of quartz, chalcedony, and calcareous spar, may almost be witnessed; and that of the latter, in particular, is so rapid, that the crystals can often be seen in calcareous caverns, such as that of Sky, forming like common salt, while it is also generated by infiltration. Chalcedony is produced in the latter way, quartz in both ; while the veins of quartz and of carbonat of lime, are generated in this manner. I have also already proved that the theory of infiltration explains the imbedded nodules of the amygdaloids ; and thus there is established a considerable list of minerals formed by means of aqueous solution. That which takes place in this case, may equally happen in a mineral vein.

Though we have not yet proved that all the other earthy saline materials, such as gypsum and barytes, are produced from watery solution, chemistry and analogy both render it very probable ; and these may therefore be added to the aqueous list, with little hazard of error ; certainly with much less than they could be referred to an igneous origin. Lastly, we may possibly also refer to the same division, those which are found associated or imbedded in quartz, as disthene is ; though such cases as this are far more questionable.

The list constructed from these various kinds of evidence will therefore contain the following minerals, and possibly many more ; and it is here divided under these several heads of more or less unexceptionable proof. I do not add those which are imbedded in primary limestone ; because it is more than probable, that these have undergone the process of fusion ; in which case their imbedded minerals must be referred, as those of granite are, to an igneous origin.

Saline Minerals.

Carbonat of Lime
Fluat of Lime
Gypsum
Brown Spar
Arragonite
Carbonat of Barytes
Sulphat of Barytes
Carbonat of Strontian
Sulphat of Strontian
Boracite
Wavellite.

With respect to some of these, it will be perceived that the proofs are complete, as they are found in the following division.

Minerals of the Amygdaloids.

Quartz ; including amethyst
Chalcedony, in all its varieties
Opal
Sulphat of Barytes
Fluor Spar
Olivin
Epidote
Mica
Chlorite
Steatite
Lithomarge
Chlorophæite
Conilite
Brown Spar
Mesotype
Nadelstein
Leucite
Sulphat of Strontian
Prehnite
Laumonite
Ichthyophthalmite
Harmotome
Analcime
Stilbite
Chabasite
Arragonite

To which may be added, as found sometimes in quartz which is apparently of aqueous origin,

Disthene
Epidote
Tremolite
Tourmalin
Actinolite

and as found in Calcareous spar,

Emerald

I have limited this list of aqueous minerals to those which are supported by the proofs above mentioned ; but if those also had been enumerated which are found associated together in cavities of veins, where one, or more, of the number consists of minerals decidedly aqueous, it might have been considerably extended. The mineralogical reader who is thus furnished with the principles on which this catalogue has been constructed, may easily extend what I need not.

In examining now the metallic minerals, so as to determine which of them may have been formed from aqueous solutions, I may first have recourse to direct experiments, and to analogies drawn from these. The ready means which chemistry affords for producing many of these substances, render these artificial proofs much more complete than in the case of the earthy minerals. The proof from nature is, as in the former case, drawn from their association with the earthy minerals already proved to be of aqueous origin. That association, and the consequent proof, is often very accurate, because the metallic is imbedded in the earthy mineral; crystallized within an earthy crystallized one, as Rutile is in quartz, or else disposed in strata of aqueous origin, such as shale and secondary limestone, which have not undergone the action of fire.

The natural proofs are not quite incontrovertible when the metallic minerals are merely associated in the cavities of veins with those earthy ones which are of aqueous origin. Yet they are perhaps sufficiently strong; particularly, as many of these are, in reality, substances which, in other cases, carry much more decided proofs with them, either from other natural associations, or from chemical experiments and analogies. As the present remarks are not offered as including a series of positive facts on which a theory is to be erected, but merely as indicating the road to be followed in attempting to explain the origin of mineral veins, any doubtful particulars can be of no moment. The observations will answer all that is intended, if they turn the attention of mineralogists to a subject which ought to have been examined by those who have proposed theories of this nature; and who, in this case, seem to have proceeded by inverting the rules of philosophy. It will hereafter be seen that some minerals, both earthy and metallic, have a double origin, or are formed both from fusion and solution; so that, perhaps, some of these, such for example as those which are included in carbonat of lime, may possibly be exclusively of igneous origin.

In examining the chemical evidence, it will be convenient to class the metallic minerals according to their leading relations of this nature; as I do not intend to investigate every complicated species or variety which mineralogists have described. The following classification will answer the present purpose.

METALS; including the alloys.
OXYDES; whether simple or complicated.
SALTS; comprising Carbonats, Sulphats, Muriats, Phosphats, Arseniats, Molybdats, Tungstats, Chromats and Silicats: or combinations of more than one of these.
SULPHURETS; simple or complicated.
PHOSPHURETS.

We do not yet know how many metals can be separated from their solutions in a metallic state; but gold, silver, copper, and lead, can be procured in this manner with great facility. These may therefore be metals of an aqueous origin. Possibly this may happen to many others; from deoxydating processes in nature which we either have not examined, or which may be unattainable in our own experiments.

All the metallic oxydes, which involve a large number of these minerals, can be procured in the same manner; at least in a powdery state. If artificial chemistry has not yet contrived to obtain these in a crystallized form, it must be recollected that we cannot, like Nature, command the element of Time. Yet perhaps the case of oxydulous iron, which may be procured from the muriat by dissipating the acid, may be esteemed an instance in point; though the application of heat is necessary for this purpose.

If chemistry has not yet formed every complicated salt that is found in the list of metallic saline minerals, it has produced so many, that we may, with little hazard of error, consider the aqueous process as fully competent to the production of the whole. That Nature can exhibit some of them in a crystallized form, such as the phosphat of iron, when we can obtain them only in a powdery one, must be referred to the cause just noticed: namely, the rapidity of our operations and the slowness of hers. As to the silicats, our acquaintance with the real nature of this combination, or the exact mode in which silica acts the part of an acid, is as yet so recent and imperfect, that no opinion can at present be given respecting them.

The igneous theory of metallic veins was supposed to be supported by an incontrovertible argument derived from the sulphuret of iron, which, it was asserted, could not be formed from aqueous solution; and the same rule was therefore extended to all the other sulphurets. So far is this from being true, that Nature does produce it from aqueous solutions, abundantly. In art, it can be procured by the mere repose of the Serum of blood, and from the decomposition of sulphat of iron by animal matters. Other metallic sulphurets may probably be formed in the same manner; it is a subject that requires to be investigated by those who may have leisure. These combinations can also be procured in the aqueous method, by means of sulphuretted hydrogen; a very probable agent in nature. In these latter cases, the sulphurets are obtained only in a powdery form, but in the former, the iron pyrites is crystallized.

Respecting the phosphurets, our direct experience is little; and I need only remark, that the analogies between sulphur and phosphorus are so strong, that phosphurets might probably be procured in the moist way as well as sulphurets.

As to the evidence from nature, derived from the intimate association between certain metallic minerals and the earthy ones of aqueous origin, the chief of these latter are calcareous spar and quartz, Barytes and fluor are less conspicuous in this respect. The union with calcareous spar is rather more frequent than that with quartz: but as these different earthy minerals, and particularly quartz and calcareous spar, frequently occur together, it is not necessary to distinguish the metallic ones which, in some cases, seem to be peculiarly associated either with the one or with the other. The following list therefore contains those which are found in these associations, arranged according to their chemical natures and under the most general terms.

Metals and Alloys.

Gold
Silver
Arsenical Silver
Iron
Copper
Arsenical Cobalt
Bismuth
Tellurium
Silver; Amalgam
Antimony
Arsenical Pyrites
Arsenical Nickel.

Oxydes.

Copper; black, and red
Iron; Hematite
Lead; Minium
Titanium; Rutile, Anatase
Arsenical Oxyde
Uranium; green, and black
Manganese; red, and black
Cobalt; red, and black.

Salts.

Silver; Muriat
Copper; Muriat, Arseniat, Phosphat
Lead; Phosphat, Carbonat, Sulphat, Molybdat
Iron; Muriat, Arseniat, Carbonat, Phosphat,
Tungsten; Wolfram
Zinc; Carbonats
Bismuth; Carbonat
Titanium; Silicat (Sphene.)

Sulphurets.

Silver
Copper; yellow, gray
Lead. Lead and Antimony
Mercury; brown, red
Zinc
Arsenic. Arsenic and Iron
Antimony; red, and gray
Bismuth.
Iron.

The minerals which seem to carry the evidence of an aqueous origin in their forms, are the following.

Earthy Phosphat of Iron
Stalactitical Hæmatites
Bog iron ore
Iron stone
Malachite
Stalactitical Manganese oxyde, red and black
Stalactitical Calamine
Stalactitical pyrites, whether of iron or copper.

The last list contains the minerals found in secondary strata, of aqueous deposition, and which do not appear to have experienced the influence of fire.

Gold
Quicksilver
Muriat of quicksilver
Sulphuret of quicksilver
Blue carbonat of copper
Green carbonat of copper
Oxydulous iron
Iron pyrites
Hematites
Iron stones and ochres
Cobalt; black oxyde
Manganese; black oxyde.

All of these are found in the preceding enumeration; so that these situations only offer proofs in confirmation of the present views.

I must now examine the minerals, whether earthy or metallic, which are the produce of igneous fusion or of sublimation from a state of vapour. The evidences respecting these are also derived from two sources; from chemical experience, and from their positions in rocks which are known to be the produce of fire. These last may be limited to granite, the porphyries and traps, and the volcanic rocks; though there seems no reason to doubt that gneiss, micaceous schist, and some other primary strata might be added to those; in which case the catalogue might be still further increased.

The earthy minerals which may be modified by artificial fire, or which undergo the action of heat without destruction, are the carbonats of lime, barytes, and strontian, and the phosphat of lime. Silica is sublimed in a crystalline form, as I have proved. Of the metallic minerals, every metal may be sublimed by artificial heat; and they all admit of being crystallized by fusion. All the sulphurets can be fused; all appear capable of being sublimed; and, probably, the whole can also be produced in this way, by a direct combination of their ingredients. All the oxydes are produced from the metals by heat, and some of them admit of being volatilized. Under these circumstances also, some of them crystallize; as is the case with the red oxyde of copper formed in the cavities of metallic vessels in Pompeii. It is probable that some of the metallic salts, the arseniats for example, can be produced in this way; but I cannot quote any satisfactory experiments on a subject which, in all its bearings, is well worthy the attention of those chemists who are interested in geology, and whose leisure is greater than my own.

In examining the evidence which nature affords on this question, the following is a list of such earthy minerals as are found in the situations above mentioned. It is probable that many are omitted; as no evidence but what seemed unexceptionable has been taken; and, in examining the entire catalogue of minerals, it will easily be found that there are some of which the origin still remains uncertain, and which are therefore excluded both from the aqueous and the igneous lists.

Earthy Minerals.

Quartz (by fusion and by sublimation)
Felspar
Mica
Hornblende
Actinolite
Chlorite
Steatite
Serpentine
Chrysoberyl
Epidote
Apatite
Pinite
Idocrase
Anthophyllite
Andalusite
Stilbite
Jade
Fettstein
Talc
Opal
Chrysoprase
Haüyne
Meionite
Sommite
Leucite
Pseudo sommite
Pleonaste
Garnet
Cyanite
Zircon
Fluor spar
Spodumene
Corundum
Beryl
Topaz
Tourmalin
Schorl
Tremolite
Emerald
Gabbronite
Wernerite
Pyrophysalite
Lapis lazuli
Asbestos
Hypersthene
Diallage
Augit
Sahlite
Peridot
Melilite
Tabular spar
Melanite
Idocrase
Ice spar
Arragonite

Together with some other volcanic minerals which are yet ill defined. And the metallic minerals thus found, are the following.

Copper
Oxydulous iron
Galena
Graphite
Chromat of Iron
Sphene
Iron pyrites
Oxyde of Tin
Sulph. Molybdena
Gold.

Such is the balance, as far as it yet appears possible to construct a tolerable list of this nature, between the aqueous and the igneous minerals. It would be highly improper, at present, to deduce from it any conclusions respecting a theory of mineral veins. For, though all the minerals of these were aqueous, or all igneous, we are equally at a loss to conjecture whence they came and how they are so limited and so disposed as they are in veins. It might indeed be considered an argument in favour of an igneous theory, that the mines of Nagyag lie in volcanic rocks. But it is evident that this fact proves no more in this case than in that of granite or trap; since, in all of these rocks alike, aqueous infiltration takes place, as well into the veins as into the volcanic and trap amygdaloids.

But it is here worthy of remark, that of the earthy minerals actually found in mineral veins, there are more of an aqueous than of an igneous origin; although there are many more igneous than aqueous minerals in nature. With respect to the metallic ones, the difference is still more in favour of the aqueous minerals. That many of both kinds have a double origin, is only one out of the numerous difficulties that beset this subject. These are, in fact, such, and so apparently unsurmountable at present, that a prudent geologist will suspend his judgment on the subject; provided he does not also suspend his investigations. Both the theories are before him, and he ought to try the facts by both, not by one only, to the exclusion of the other. In this pursuit he ought to take into his views the formation of minerals by sublimation, and their production from infiltration; two processes which have been neglected by former Theorists. Not however that these will, on either side, form, in themselves, a theory; because, even were there not many more circumstances at present unintelligible in veins, we are still unable to explain whence, on either hypothesis, the minerals have arrived at their present places. This last objection applies also to a proposal which has been made towards explaining the formation of mineral veins, by means of galvanic actions occurring between the vein itself and its walls; though it is not impossible that such causes may have aided in producing their disposition, or modifying the minerals themselves.

CHAP. XX.

On the geological Relations of the organic Fossils.

Of all the appearances which the earth presents, nothing has excited more attention than the existence of animal bodies in the strata; while the air of mystery which attended it stimulated curiosity, and may be said to have laid the foundation of Geological science. If the presence of animals, once submarine, in rocks and on lofty mountains, was a cause of wonder and a source of theories, so did the discovery of the bones of large animals lead to the belief of pre-existing races of giants, while, in both cases, philosophy, with history, sacred and profane, were perverted to find explanations.

The increase of knowledge has given a very different complexion to this subject, and a more rational direction to the pursuit. Yet the Geologist seems in danger of forgetting that it is but one part of his science. Its details belong to zoology and botany; and he loses sight of his main object when he pursues these minutiæ to the neglect of their more interesting connexions with the history of the globe. Still more deeply does he err, when he imagines that a theory of the earth can be founded on what involves so small a portion of it's structure and history. It is doubtless, essential to know these objects; as, to arrange and name them is the grammar of this department. But it is unfortunately true, that whether the contemplation of minutiæ disables the mind for wider views, or that only a minute mind can be engrossed by such things, the power of profiting by collections and their study, diminishes in proportion to their extent and the activity of collectors, whether it be in natural history or books.

The true business of a Geologist, here, is of a far higher character. It is to determine the antiquity of these objects and that of the earths in which they lived, the waters which they inhabited, and the former places of those; to explain why they are now imbedded in rocks when once free, why elevated on the land when once beneath the sea, why they are partially distributed, and far more; as it is also his office to see how these things explain the history of the earth. If found in alluvial soils, other inquiries of an analogous nature arise, relating especially to the later history of the globe. And in the study of the objects themselves, if he undertakes the office of the zoologist and botanist, it is his business to compare the dead with the existing races; through which it is his own proper office to draw inferences as to the history of the living creations of the Earth, as to that of the Earth itself.

The limits and nature of an elementary work on Geology, do not permit an examination of this subject as it belongs to Zoology and Botany: a treatise would be demanded for it, and that would also be a large one. I have already published a skeleton for such a work, or a basis on which those details might be engrafted: as it is now time that they should be collected and embodied. For the objects themselves, I must refer to that and other well-known books; especially to local records, and to professed arrangements of fossil organic bodies; here, I must confine myself to purely geological science, as much as possible. And that will also be more useful; for while books abound on the fossil bodies themselves, their geological bearings and connexions have been almost entirely neglected in those works, as, from that neglect, many erroneous conclusions have been drawn respecting the history of the earth, and also of those objects themselves.

It may interest those who think the antiquities of a science worth studying, to name some antient opinions on this subject: the record of folly would be more valuable, did it teach ourselves wisdom. But when we smile at Ray and Lister and Bertrand and others, we forget the greater folly and ignorance which create and destroy oceans, dissolve rocks without water, and fuse them without heat. If these fossils were thought to be formed by a plastic power in Nature, this was but the Greek philosophy which Cudworth had borrowed. If Lusus Naturæ was a term without an idea, cheating by the semblance of knowledge, we are doing the same every day, and forget to note it. If the seeds of shell fishes were evaporated and conveyed to the earth, modern physiology does not yet see on how many points it will hereafter be an object of equal ridicule. If stones themselves grew from seeds, a yesterday's philosophy produces a man from a monas, and dares to smile at the self-growth of organic fossils. Forms uniting the organic with the inorganic world, materials prepared for living beings, these, and more, can be paralleled by modern hypotheses. Voltaire's reading might however have taught him what his knowledge of nature could not; since Herodotus, Strabo, Pliny, and others, had, long before, formed just conclusions on this subject. The parent philosophy was wiser than its progeny, for many centuries, in this and far more. But let Tertullian have the credit which he deserves, for explaining the positions of organic fossils through the deluge. This was a bold and a grand view: but it is ours to remember that no philosophy will endure which is not founded on truth. Enough.

Of the general Division and Nature of organic Fossils.

The great and obvious divisions are into marine and terrestrial, since these materially concern the history of the earth as explained by these bodies. But, under the same reference, the last must be separated into aquatic and terrene. As organic bodies, they are divided into animals and vegetables; the latter being rare among the marine, and confined chiefly to the terrestrial deposits: the former being both marine and terrestrial, and, in the latter division, terrene and aquatic.

The Lithophytes, or corals, among the marine fossils, form the basis of strata of high antiquity; having originally perhaps resembled the coral islands of our own day. That they should be mixed with other shells, we can easily understand; and, when found independent, they may belong to alluvial marine deposits, or be derived from demolished rocks. The testaceous animals constitute a much larger division, as their produce in rock also far exceeds that of the former; and thus also do they occupy a much greater range among the strata. The crustaceous animals are comparatively rare, in quantity and in variety. This is explicable, partly by retrospective inferences derived from their present known rarity, and partly by the tenderness of their structure. And this last fact explains the still greater rarity of the fishes, of which the hard parts are often the only ones preserved. If the cetaceous fishes are also rare, we must recollect that they are so as living animals: and, of these and the last, it may be remarked that they are generally found in the more recent strata, and commonly under peculiar circumstances formerly explained. Rocks of undisputed marine origin do not often contain amphibious animals, nor do these abound anywhere. There are however tortoises in the chalk, and animals of the Lizard tribe in the Lias; itself an amphibious deposit, as I shall hereafter suggest; at least where it contains such remains.

Thus does this race connect the marine and the terrestrial organic fossils of an animal nature. Among the latter, the lithophytes are rare; and if millepor æ occur in the coal strata, the inferior beds of this are amphibious. Testaceous animals are found in the fresh water strata, just as in the marine ones; but they are more limited in variety and quantity, and the rocks themselves belong either to the coal series or the tertiary deposits; otherwise than under transportation, and in the amphibious lias. Fishes are also found in the same strata; and I have formerly pointed out the sources of confusion and error on this subject.

The fossil remains of terrene animals form a large class, in variety, if not in numbers; presenting also a peculiar interest, by approximating the existing earth with its last preceding condition. If we extend the rule so as to consider every buried animal as a geological organic fossil, they preserve the chain unbroken. By transportation, they occur in marine strata; and they also exist in lacustral ones and in alluvia. Former chapters have explained much that I need not here repeat on this subject; and it only remains to add, that they are sometimes found in modern rocks, produced like Travertino, or from fragments. The remains themselves comprise quadrupeds, birds, reptiles, and insects, to which I may again add amphibia.

If it was once thought that the Elephant, and other remains of Italy were derived from animals introduced by the Romans, that hypothesis has passed away. Insects could not be common; but they are likely to prove more numerous when the lacustral deposits to which they belong are better known. They have been found at Nicholschitz in Moravia, at Radeberg in Croatia, at Sinigaglia on the Apennine, and elsewhere; consisting of coleoptera, hymenoptera and diptera, and including Libellula, Cimex, grashoppers, and others.

Vegetable fossils are very rare in the marine strata, as marine vegetables; transported terrene ones are far more common, and must not be confounded because of their situation. Yet confervæ and fuci have been found in the limestone of the Alps and Apennines, of Transylvania, and of Christiania in Norway. Compared to these, the terrestrial fossil plants abound in variety and numbers: occurring where they might be expected, in the coal strata chiefly, and also in the tertiary lacustral deposits. The former are palustral, if not aquatic, and are preserved in their places. They must be distinguished from the more properly terrene ones which have undergone transportation, and which occur in the alluvial deposits often confounded with the tertiary. And I must lastly here include the superficial fragments of vegetables petrified, such as occur in Antigua and at Madras, in Africa as described by many travellers, and in New Holland, as mentioned by Collins.

Of the Conditions in which organic Fossils occur.

Shells are often found in sand, dry and fragile, as if calcined; having lost their animal matter. In other cases, that animal bond is preserved, as are even their colours, well known in the Lumachella marble; while similar variations occur in the case of bones and teeth.

Sometimes, the most delicate parts of the shell are perfect, even to the membrane of the hinge, while in others they are compressed or broken, and even dissevered and dispersed. The causes of all this are sufficiently obvious. The animal matter of fishes is rarely preserved, as I already noticed; and, of the quadrupeds and cetacea, only the bones are found, except in the rare and peculiar cases elsewhere described.

Though casts and impressions cannot be called organic fossils, they are equivalent records. They need not be here distinguished, being essentially the same. In the case of leaves, there is often no other record. In shells, the interior cast is a model. It is often difficult to account for the disappearance of the organic body in these cases; as the stone must have been previously indurated, when the cast is perfect, and we do not see how that should remain when the model was destroyed. In the calcareous rocks, where the shell has combined with the stone, there is no difficulty. And in this case also, the animal matter is sometimes diffused through the rock, producing the fetid limestones; a fact occurring too in the case of fishes. The ammonites are sometimes filled with sand, and the shell itself is silicified: yet when a shell is filled with flint, it remains calcareous, and is sometimes crystallized, assuming at least a fibrous structure. In the former also, the siphunculus sometimes remains when the shell has disappeared.

In the ligneous fossils, the wood is replaced by sandstone, but the bark often remains, converted into coal. Such are the well-known fossils of the coal strata. In some cases, especially in the minuter fossils, the wood is converted into charcoal. Leaves, or at least their bark, have become coal: they are often, rather drawings than even bas-reliefs of the originals. In other cases, as often happens in shale, the bituminous matter is diffused through the rock, and nothing but the impression remains, sometimes with a mere film of charcoal.

What is called petrifaction is another condition of organic fossils, much more rare, if we adhere to a true definition. Where this process is perfect, the organic body has disappeared, but its form is preserved in the stony matter, often to the very anatomy; especially in wood and coral converted into chalcedony. In other instances, the process is incomplete, or a part of the original remains, easily discoverable by sulphuric acid, when not visible, especially if it be a vegetable; as, in shells, where those become chert, the animal matter is sometimes united to the flint, and can also generally be detected by chemical means.

Each of these classes of bodies are petrified by siliceous and by calcareous matter. The former is either quartz, flint, chert, chalcedony, or opal: and the other may be pure carbonat of lime, as it may also be argillaceous, or cherty. If shale has been considered as a petrifying substance, its produce is more properly ranked with casts. If metallizations, as they are termed, are to be included here, they are nearly limited to pyrites in the animals, though sulphuret of mercury has been mentioned. But bog iron ore and phosphat of iron may be also ranked with these in the case of vegetables. I know not that bituminization should have a place here; but it is at least an analogous fact, to which some writers have given a place. Lastly, it has been said, that ostreæ, gryphites, terebratulæ, corals, and serpulæ, have commonly preserved their shelly matter, when porcellanites, volutes, and others, have lost it, that echinites and encrinites are, particularly, converted into calcareous spar, that ammonites and others vanish, and that belemnites outlast those which they accompany, presenting also a radiated crystallization. But these facts are not sufficiently generalized to allow of any conclusions: if there are such leading effects, we must probably seek the causes in the history of the rocks themselves, not in the nature of the organic bodies.

Incrustations must not be confounded with petrifactions. In this case, the organic body is simply involved; and these are also generally of recent date. On the great scale, they are all calcareous, and are without interest, except where they occur in the travertinos of Italy, where they have often given rise to errors of moment. But there are two of a different nature which demand notice, though the interest is of a limited nature.

I have proved, in the Geological Transactions, what had been denied, that minute vegetables were preserved in chalcedony, as I have there equally proved it of insects. These are the so-called moss agates: but care must be taken not to confound chlorite with these remains, it being the cause of the appearances resembling confervæ; crystallizing thus, by the continued superposition of its scales. In my collection, three or four mosses, one possibly a Jungermannia, and a Lichen, admit of no dispute; as it is equally easy to account for the fact, by the familiar infiltration of chalcedony into open cavities. In the same manner, insects may be entangled; and thus have two examples occurred, my own a lepidopterous Pupa. It is also said that an insect has been found in menilithe. Insects are sometimes also incrusted with amber, as is familiar. The remains in chalcedony may be of any modern time: but those in amber must be as old as the lignites of the older alluvia with which they occur. The explanation of this is, also, easy: the substance is the resin of former trees, bituminized, as the wood has been: and this explanation of the real nature of amber, so long disputed, is confirmed by the semibituminization of similar resins in later strata, accompanying, similarly, semibituminized wood, as I formerly explained in different papers on these subjects. The existing resins, equally including insects, resemble these fossil substances so much, as to be constantly sold for amber, and not always from fraud, but from ignorance: but the test is easy, as I have shown in a paper on this subject in one of the journals.

Of the Rocks in which organic Fossils occur.

This forms an important question in geology: they abound in some, and are absent from others; while it will be anticipated that they should occur in the stratified, and be excluded from the unstratified rocks. And such is the fact. No one has imagined that they could occur in granite; but they who desired to make the trap rocks of aqueous origin, have pretended to find them in those. It is necessary to show what the error and the truth are. Nor can they be expected in Serpentine or in Diallage rock; for I have proved that these also are rocks of fusion.

They may exist in the tufas of this family, because these are conglomerates, often transported, and often aqueous: It is the very case of Pompeii in another form; and thus is bituminized wood found under and in the solid trap of the Western Islands of Scotland; but when in the latter case, it is found in the midst of entangled tufas. When said to have occurred in basalt, this is the error of ignorance: the indurated shale, or Lydian Stone, beneath it, has been mistaken for that substance, as I have proved. If a shell should even be found in a real basalt, as has been said to have occurred, this is possible, because basaltic veins often entangle fragments of limestone; and might thus include a fossil shell also. Such an occurrence will probably however prove rare, as it will also leave the present rule intact.

If, now, the stratified rocks do not always contain organic fossils, the reasons for the exclusion will immediately appear, in each case. Generally, these are, that some strata may have existed before the creation of organic beings, that some have undergone changes destructive to them and to their remains, that some earths are unsuitable to their habits as places of residence, that deposits of stony or earthy matters must often have been made in too short a time to permit of their multiplication, that even the present sea does not every where contain the living beings, and that there must have been antient situations and circumstances where they did not exist as such.

Their existence in the strata is easy of explanation. The marly deposits of a lake, or an oyster bank in the sea, are the preparations for future rocky strata of organic fossils, as their powder forms compact limestones, and their sand oolithes; the latter produced daily under our eyes. It is equally obvious that they must predominate in limestones, since they have generated these, and that they should occur in shale rather than in sandstone, because living shells do the same, or inhabit mud in preference to sand. If they are colonial in rocks, so are they in the sea; if intermixed, we have still their living models, in a state of intermixture. The secondary marine strata are thus their principal seats; and why they should occur in the tertiary ones and the alluvia, is too obvious to require a word. If, in the days of geological ignorance, they were thought limited to the secondary strata, that time is past; though, ever unwilling to surrender an hypothesis, and tenacious of it in proportion to its weakness, Geologists have invented the term *transition*, partly to preserve this fanciful rule. They are abundant in the latest argillaceous schists and limestones of the primary series; drawing that line at the old red sandstone.

In proceeding to consider the individual strata, I may first remark that organic fossils have never occurred in gneiss or in micaceous schist; as was to be expected, at least in the predominant cases, from the theory of those rocks formerly given. Yet, as the latter rock is sometimes formed of fragments, it is possible that they should be found in it. We have no right to decide on the non-existence of animals during this early stage of the earth, because we have not found them. Though this rock has been exposed to heat, the shells of shale are not always obliterated under the same circumstances. If we make rules of this kind, we shall never search, and never know: when we decide from negative evidence, we make our ignorance the measure of what is. This negative evidence as to an early creation is otherwise worthless; because, even in the secondary strata, organic bodies are seldom found in siliceous rocks, and because there is an evident comparative rarity of animal fossils in retroceding, as would be proved by the scarcity of primary limestone, were there no other evidence.

This is the hypothetical suggestion of a doubt as to one of the great rules of Geologists: it must now be asked whether there be not evidence from which to conjecture an organic creation of even this early date. They have been unwilling to seek it, on account of their hypothesis, or even to admit the possibility, when produced. Anthracite is coal, and it is found in the oldest rocks; while, under this hypothesis, it has been esteemed a pure and proper mineral substance. It is not carbon under its semimetallic, and therefore more obviously mineral form of plumbago, at least; yet even this might be the produce of vegetables, since it is daily formed from coal in the Iron-furnace. If Anthracite is not to rank with vegetable coal because it contains no bitumen, this is equally true of undoubted vegetable coals, in Pennsylvania and elsewhere. And therefore the early anthracite may be, and probably is, an organic produce, and the record of an antient vegetable creation; while its very rarity is an argument in favour of this supposition.

Calcareous concave bodies, precisely resembling fragments of shells, occur in hornblende schist in Scotland, in Glen Tilt, not distinguishable from those of the basaltic shales: had they been found in a modern shale, no question would have been entertained as to their nature. This is the more remarkable; as, under no other circumstance, does lime, as a visible ingredient, occur in this crystalline rock of fusion. I have proved that hornblende schist is a fused slate; and thus might it as well contain shells, under that fusion, as the Lydian stones, equally fused or semifused, so often do. This is a second evidence of an early creation, the first, of vegetables, and the second of animals; the one supports the other, and they are confirmed still further by a third fact. These are, the existence of orthoceratites, abundant in a quartz rock in Sutherland, which follows gneiss and is connected with it. And I consider this evidence to be as perfect as we are entitled to expect on such a subject, from the researches of one individual, under the neglect of all other observers. In any case, where no previous hypothesis to the contrary had existed, no one would hesitate in receiving it.

If a recent geologist has undertaken, in the last case, to explain otherwise what he never saw, because he preferred antient ignorance to modern truth, he has also forgotten that the secondary and the primary strata never can undergo a joint undulation: he could not have been ignorant to this extent, though his researches had been confined to the recent strata: it has been the oversight of anxiety for an hypothesis. And geologists would be better employed in seeking for new truths, than in determining what is, because they think that it ought to be. If this is to be the rule, all observation is useless: he who has seen nothing becomes the interpreter of everything, and interprets as he had previously statuted to do: Geology is attained, and we may rest from our labours. But if it is thus to become a Science, it is the first that ever succeeded through this road. I owe this remark to the Geological student, whom I have undertaken to teach in all that I know myself, and whom it is my duty to guide where he has been misled.

Organic fossils ought to be found in the primary limestones; and they are so, if we exclude the false distinction already noticed. If they do not occur in the oldest ones, the explanation is easy. This substance is remarkably fusible; and I have shown that it has been locally fused by Trap, in Sky and the Isle of Man, to the obliteration of those fossils which abound in other parts of the same stratum. And the earlier limestones have been most exposed to heat. The occurrence of organic fossils in the argillaceous schist is notorious. And this terminates the primary series: while the conclusion is, that it does contain organic fossils, abundantly in its upper parts, with sufficient indications in its lower ones; thus pointing to a living creation as old as any rocks that we know, while the causes of a scanty evidence of this kind are obvious. We may allow that the living animals were less numerous and abundant at the earlier periods; and the rarity of primary limestone is a sufficient proof of this; but that is all. And if also the whole of the primary series was formed beneath the water at one period of repose, as appears true from the parallelism of the strata, it would be an extraordinary conclusion that a creation should have occurred late in that period, and not at its commencement, as has been the case apparently in all the successive ones.

In the secondary series, organic bodies are rare in the lowest sandstone. And this is intelligible, because, at so early a date after a great revolution, they could not have abounded in the ocean; while it is probable also that much of this deposit consists of antient terrestrial alluvia, produced in a manner I shall hereafter explain. Hitherto, with the probable exception of Anthracite, every fossil is marine; and this continues through the next, or mountain limestone, where they become abundant; because, like all other secondary limestones, it has been formed from their remains, as it could not indeed have been produced from any other source. But here the exclusion terminates, and here also commence terrestrial vegetables as well as animals. Whether they existed as living ones before this, we do not know; and, again, ought not to decide that they did not, from negative evidence.

The coal strata are here the great depository of terrestrial organic fossils; of plants and fresh-water shells: and I need not distinguish these strata further, than to say that the fossils occur chiefly in the shales, and in the limestones, when any are present; as vegetable fragments in the shape of charcoal are found in the coal itself. In the magnesian limestone, they are limited in number, and are again marine. The red marl sandstone, like the inferior one, contains very few: and there may be special reasons for this, since there are peculiarities respecting this stratum, which I shall speak of hereafter. I have already noticed the intermixture of terrestrial and marine remains in the lias series; and if it is at present one of the supposed geological difficulties, this will be a subject for after inquiry. That series has been more boldly described than well understood; as it has been confidently transferred to other countries, under the favourite hypothesis of universal formations. It is scarcely necessary to proceed further in the same detail. Organic fossils ought to occur abundantly in the equally abused and hypothetical Oolithe, and they do so: as they do, much less abundantly, in the no less hypothetical Green sand, becoming, finally, abundant in the Chalk.

I have only to remark lastly, of all these strata, that these fossils are occasionally absent in some places while abundant in others; but that all these differences can be explained through simple considerations, which, after all that I have already said, it would be superfluous to suggest.

So recently as I have described the tertiary formations, I need not repeat what concerns them in this respect. The alluvial soils contain the chief remains of the larger and more perfect terrestrial animals, yet very partially situated; and I need not here distinguish among the qualities or origins of these, nor at present notice such remains of mixed origin as they may include. With the loose alluvial matters may also be comprised what I have here termed alluvial rocks, however existing. And those which have been formed in fissures or cavities, or in any other manner, from cemented fragments, such as the organic rocks of Gibraltar and of Dalmatia, abound in the remains of terrestrial animals, as the Travertinos often contain vegetable and terrestrial shells. If, lastly, I may include among the alluvial deposits, those loose substances found in caverns, these are also the frequent notorious repositories of some of the most remarkable animal remains which have been discovered. Arriving above the later gravel and sand, and at the peat, the fossil remains introduce us to our own living world. And thus also are we introduced to it by the fossil shells of recently drained or existing lakes, and by those terrestrial and superficial deposits of shells, resembling soft chalk intermixed with these objects, the existence and nature of which I first pointed out in Perthshire, to those who have, many years afterwards, made the "original discovery." The celebrated human skeletons of Guadaloupe are the same geological fact in another form: a recent oolithe is but an indurated alluvium.

Of the Order of Succession and relative Antiquity of organic Fossils.

It has been supposed that a certain order existed among the species of organic fossils, under relations to a definite succession among the strata; but this includes two points of considerable importance, the one relating to the organic creation itself, the other to its geological relations. The last will form a separate section hereafter.

I shall not insult the common sense of the reader by retailing the dreams of writers respecting organic molecules and the gradual improvement of species. For this miserable progeny of an atheistical Greek philosophy, he may consult Lamarck, De Maillet, and others: to be ingenious were something, and there is attraction in novelty; but fiction and folly that are alike dull and stale, do not deserve even criticism. But more sober inquirers have conceived that the earlier species of animals were less perfect than the more recent, and have imagined a gradual improvement in their organizations; as they have further presumed on a gradually increasing number of genera and species. These questions must, however, be determined by facts, not by speculative reasonings. It is plain that any theory of this nature must labour under the radical fault of deficiency of evidence. Though we follow a succession according to the order of the strata, it teaches nothing on these subjects, because of its imperfection, particularly in the more remote strata, where we know not what has existed. If we even suppose, as has been said, that the first animals were solely marine, and limited to corals and shell fishes, there are not facts to prove this. All the strata were formed under the sea, and could contain only marine remains, except under rare accidents. It will be so in the rocks now forming. The animals thus supposed exclusive have also peculiar powers of durability, while the higher organizations are perishable; especially so under such changes as the earlier rocks have undergone; so that, for all which this imaginary evidence proves, the earlier ocean might have teemed with as great a variety of life as the present one, as there might also have been an inhabited earth. I repeat what I have often urged before: we are trying to measure truth by our own ignorance: the usual proceeding of ignorance united to vanity.

But the question as to land animals must be further examined: a stronghold has here been erected on mud, in a literal as well as a metaphorical sense. With the few exceptions already noted, their remains are now found in alluvial soils, on the land; and it could not have been otherwise in former times. If rivers deposit the materials of future rocks under the sea, they very rarely carry thither the bones of land animals. The geologists of a future earth might determine, as we do now, that marine animals alone had formerly existed. If animals are to be preserved in rocks, for future ages, they must be preserved where they died. Hence is it that we find the remains of amphibious animals, and not those of terrestrial ones; for the same would happen, or does happen, at this very day. They were enclosed at once in mud, like shells, and preserved from destruction. And hence the simple solution of the wonderful mysteries of the Lias and its Lizards, which are to prove equally wonderful mysteries about an antient earth. There was a dry earth long before and long after these beds; or whence are the strata from Coal to Chalk? He who can believe that this earth contained nothing but Lizards, and during one period only, has thought too much of his specimens: what could be preserved has been embodied in stone, and all else has perished: it has ever been so, and ever will. The preserved animals differ from the present; that is all; but that is a separate question.

This was the essential preliminary question; for it was that of the value of the Evidence. If we now examine the preserved animals, they do not prove any thing as to a successive improvement of organizations. There are identical or corresponding genera in the most antient and the most recent strata. Echinites, chamites, tellinites, ammonites, and others, occur in the primary schists and in almost every one of the superior strata. Madreporites, terebratulites, belemnites, and orthoceratites are found in the oldest sandstone and in the chalk. I might fill a page with such examples: but the reader can do this from the catalogues of those who have asserted the very reverse. As far as such imaginary improvement concerns terrestrial and large animals, the answer has been already given. All the evidence is against the hypothesis in the one case; and, in the other, there is no evidence but that which, as negative, is nothing.

The assertion as to a numerical increase of species labours under the same want of correct evidence. We do not possess the species: and the known comparative destruction of the more antient ones is a fatal ignorance as to any conclusions. If any one supposes a gradual and successive creation of species, he ought to give reasons for that which is a metaphysical and theological conclusion: it is a subject on which we have no facts but the history of our own Creation, and that does not countenance such a theory. We can understand an increase after each revolution, and this is highly probable; but those augmentations must be limited to these changes and new conditions of the earth's form. As far as the visible facts exist, there is an increase between the earliest and the latest; but there is no evidence of a gradual one, since there are more organic fossils in the primary schist than in the red marl; to adduce no other facts, quite familiar, such as the Lizards of the lias, the vegetables of the Coal strata, and so on, wanting in every stratum beyond these. The increase of numbers, which is a fact, on the whole series, though not a fact as gradation is concerned, is understood from what has been formerly said.

It is asserted that genera as well as species change with the strata. That is not true; the answer is given already in comparing the lower and the higher; it might be rendered overwhelming from any catalogue. But this question will recur in another section.

Respecting the relative antiquity of different animals, it is imagined that the oviparous are more antient than the viviparous quadrupeds, and that they existed together with fishes only. And because of certain successions of these found in the tertiary strata of Paris, different imaginary dates have been assigned for certain sets. I hope that I have answered what relates to the oviparous animals: yet it is painful to see an able philosopher drawing such conclusions from such facts. But there seem to be no limits to the influence of hypotheses, when they could make even this philosopher forget, in his own peculiar department, that a hog possessed a divided hoof.

Of the Connexion between Fossil and living organic Bodies, and of Extinctions.

If it was once thought that no fossil organic beds corresponded to a living one, this supposition has been disproved. But it is said that this correspondence is little or nothing as to the more antient species, and that the resemblances increase in proceeding upwards, till in the most recent, the fossil and the living are identical. But the evidence is unsatisfactory, on nearly the same grounds as before, our ignorance, and chiefly as to what is existing in the present ocean. It is also not philosophical to seek for living resemblances to fossil bodies in the nearest seas, and to decide on the extinction of the latter from the absence of the former; since we do not know what the past conditions of the earth were. Recent researches have discovered more resemblances than were once thought to exist, and further ones will probably add to these. If indeed the remote revolutions destroyed all the existing races, we might conclude that all the distant ones were extinct; yet we could not even then prove the asserted want of correspondence, without assuming, what we can never know, that new ones were not produced on the same models. The former is a difficult question of geology; the latter is a purely metaphysical and vain speculation. It is better to examine the facts, imperfect as they are, and to rest on them for the present. Yet there are difficulties and uncertainties in such investigations, founded on the ignorance of naturalists as to the genera of Nature; while they perpetually forget that their own are often artificial, the conveniences of Nomenclature; even forgetting their own laudable anxiety to discover the real plan of Creation.

A few facts must here suffice. Echini, terebratulæ, turbines, chamæ and tellinæ, occur as existing genera, and are also found in the primary argillaceous schist. Anomiæ, patellæ, nautili, and crabs are living genera, and also fossils in the lowest limestone. In the lias, chama, donax, helix, trochus, asteria, and many more, are living genera; and proceeding upwards to the chalk, we find, of the latter, balanus, pholas, buccinum, turbo, patella, pecten, murex, pinna, and others, together with tortoises, besides the amphibia formerly mentioned. Any catalogue will furnish further generic identities. That of De France, gives, among other conclusions, a hundred and ninety six genera living and fossil both, and a hundred and fourteen fossil only. Whatever interest to zoology the remainder may possess, they have no great bearing on the subject before us; and when a contrast is drawn between the number of corals and shells in the fossil state, and those of the pteropodes and others in the living one, it is plain that this is a question of preservation simply, not of existence. But the reader can consult for himself. I selected the first part of the short preceding catalogue, because each set lies beyond great revolutions of the earth, and the former beyond three at least. If, instead of genera, it is to be a question of the correspondence of species, the evidence is imperfect, for these reasons. The fossil animals are rare at distant periods of the earth, the specific distinctions are often destroyed, and we do not even know the existing species, without which all evidence is nugatory. And accordingly, with the exception of three or four, chiefly terebratulæ, before the chalk, there are no correspondences till we arrive at the tertiary strata, while for these, cumbersome here, I must also refer to the catalogues. And let it also be remembered, that we have no right to reason as to the entire globe from a limited spot; for this cannot possibly be a rule for the whole earth, whatever an indolent convenience which decides without examining, or an hypothesis which knows before it has learnt, or "National vanity" may think.

Of the larger animals, the most recent of fossil remains, and comprising upwards of eighty species, eleven or twelve are thought to be existing, sixteen or eighteen are supposed doubtful, and the remainder have no known parallels; while there is now no great probability of discovering many new quadrupeds of large size. But it is difficult to perceive what relation these latter facts possess to geology. They seem pure questions of Zoology: but, unfortunately, geologists have intruded too far into this branch of natural history, led on from the earlier fossils, and thus must they torment every fact to suit some geological hypothesis. In Cuvier, and in more recent writings on this subject, the reader will find the zoological details which do not belong to a work of the present nature.

Geology however is interested in inquiring into the causes of these disappearances, though probably less concerned in them than some visionary writers have thought. In such cases as those of the lias, the solution is, obviously, to be sought in early peculiarities of the earth's surface. It is also easy to understand how animals should have been destroyed by such partial revolutions as those of Italy; as that may explain some facts of this nature respecting the basin of Paris, presuming, that this has undergone similar or analogous elevations. It is plain also, that no such change could have taken place in Italy, without affecting, perhaps all Europe, and even much more, in the neighbouring continent; since far inferior occurrences of this nature in South America have produced wide destruction: while it would not be a very extravagant supposition that the elevation of the Paris basin, that of the Isle of Wight, perhaps of Auvergne, and probably of other spots yet to be discovered, were all connected with this great partial revolution, in some manner, though it is not necessary that it should be limited to one short period. I forbore to suggest this formerly, lest I should alarm the reader with too many novel suggestions at once: but if true, it is the explanation of the loss of species and genera both, as, even in a more limited manner, it might have produced either, inasmuch as many animals are limited in extent of residence. Such a catastrophe in

New Holland at present would produce this very effect. An entire collection of animals, distinct also from every other, would be extirpated from the earth.

But it would be wrong to apply such causes or speculations too widely. Many animals are now becoming more concentrated, as they are also diminishing in numbers, chiefly from the progress of man and the changes of the earth which follow this, in change or limitation of food, if possibly also, in part, from their mutual interferences, from epizootic diseases, from changes of climate, the destruction of forests, and perhaps other causes, unknown to us. And thus, probably, without any geological catastrophes, have the numerous extinct animals of particular regions, such as the elephants of Germany and Siberia, and our own lost quadrupeds, disappeared. Hereafter, I shall have occasion to recur in some measure to this subject.

It is more easily to be understood why there should be no correspondence between existing and fossil plants. The latter belong, if antient, to the Coal Series and the lias only; and each of these has undergone revolutions which must have destroyed all germs. An ocean might have protected what the earth could not. And accordingly, if analogies have been pointed out, that is all: though it is plain that genera at least could not be thus traced, since the characteristic marks are not preserved. As to the tertiary strata, identities are probable, and, in the alluvia confounded with these, certain.

I may conclude with this general remark, that the correspondence of fossil genera or species with living ones is scarcely a question of Geology. The revolutions of the earth are proved by safer evidence. There may have been new creations following revolutions and extinctions; but this is a question of the natural history of living beings: as it is, very obviously, to inquire whether the Creator acted in one manner or in another, when we seek to determine whether such new creations differ from or resemble former ones.

Whatever remains as to supposed extinctions of races may be made brief; and like almost all which has preceded, it is chiefly a question of Zoology. The ammonites, the belemnites, and the orthoceratites, are the genera which seem most remarkably to have disappeared, yet the evidence scarcely exceeds a probability. As far as geology is concerned, it is in the causes: and these, with the facts and the far more satisfactory evidences of revolutions, have already been discussed, or will form the subject of the following chapter. It is sufficient here to say, that the first elevation of the primary strata, furnishing the materials of a next set, a second elevation which raised these above the water, the depression after the productions of the germs of coal, and the subsequent elevation of all that is now above the ocean, to note no more at present, must have produced effects on the inhabitants of the earth of whatever nature, whether these revolutions were partial or general, sudden or tedious, which could not have been but destructive, whether to extermination or not; though an entire extinction would not exclude the repetition of similar beings. And it must be through the proofs of these and the proofs of their nature, that we must draw our conclusions as to extinctions and renewals of living creations; since the organic fossil bodies themselves cannot teach us what we wish to know.

And I may end with these remarks. The negative evidence respecting the most antient races, of whatever nature, can prove nothing; and there may have been equivalent creations, or an equivalent creation, during every condition of the globe. It is probable that there was a creation more early than the earliest primary strata, but it does not appear to have been so abundant, while the real criterion lies in the limestones. There is no reason to infer a progressive creation during a period of repose, and there is reason to infer renewed ones; though there is none for concluding that such new one entirely differed from any former or the next preceding one. There are evidences of revolutions so general as to have probably destroyed all living beings; but there are also evidences of partial ones, of which the effect, on these, was also but partial. And this is particularly true of the last of all. Lastly, there is no proof of a progressive improvement of organizations, because the only evidence is negative.

Of supposed Changes of Climate in the Earth.

Geologists have thought fit to suppose that the temperature of the Earth had undergone changes since its more antient periods, or that the climates had been interchanged during its progress; and they have founded this opinion on resemblances between the organic fossils of cold climates and the existing organized bodies of warm ones. The question as to the fact depends therefore on the value of the evidence; and I hope to prove that it is worthless.

If the plants of the coal strata resemble living intertropical ones, and this be judged evidence of an interchange of climate, it ought reversely to be shown, that the fossils of the hot climates resemble the living species of the cold ones; which has not been done.

If it is meant to prove that the present temperature is less than a former one, all organic fossils should resemble those of hot climates, which cannot be shown. Such creations being admitted to be distinct ones, the form in question might as well have existed as any other: there is no necessary relation between that of a palm and a high temperature: it is a question of sensibility, and that is of arbitrary appointment.

But the following argument is decisive as to these vegetables, under both suppositions. Coal is the produce of peat; and this substance cannot be formed in hot climates, nor ever is formed from the present palms and ferns. Nor could the plants themselves have been preserved as they are, in a hot climate; because they are not so preserved between the tropics. Except through inundations, no fossil vegetable can ever have been produced in such temperatures, while those of the coal strata have been slowly accumulated under exposure to air and water.

If it is true that living corals abound in hot climates and are rare in cold ones, and that the latter possess them in the fossil state, the same answer is valid. There is no necessary connexion between such an animal and a high temperature; the appointment is equally arbitrary; and as some also are now found in cold climates, so might millions have existed in them formerly. It is the same as to shells; while in these also it has now been found, that many fossil ones of cold climates, formerly thought intertropical, belong to genera in the neighbouring seas. It is an argument from analogy, that the Elephant and Rhinoceros have actually resided in cold climates in former times; since they are thus found deposited where they died, not transported, in frozen Siberia. Geologists have formed their theory before the facts; and when these have occurred, they could not bear, as usual, to abandon them.

Volta's imagined evidence from Monte Bolca is not fact: it is one of those statements of bold ignorance on which almost all geology has hitherto been founded, as it is a specimen of that geological marvellous which has always been so attractive. It is as incorrect, too, in zoology, as in all else. Out of his hundred and five species, Blainville has extracted but ninety; and there are probably not more than sixty in reality. And instead of having been collected, as he asserts, from Asia, Africa, and America, they appear all to be inhabitants of the Mediterranean; as is probable from the account of this deposit formerly given. If that should not be rigidly true, such a revolution might have destroyed, in that sea, many that are still existing in tropical seas. This witness would long since have been cross-examined as he now is, if geologists were more anxious for truth than systems.

If the Elephants and other animals now buried in frozen climates have led to the same conclusions, this, as I have just suggested, proves no more than shells or corals do. Besides, the species are not always proved to be the same; and though they were, the dog, the horse, the fox, the hare, and many more, are found in all climates; of convertible habits, like man, or originally appointed universal. The rhinoceros is buried in Siberia, in the frozen earth of the Wilui, in proof that this climate was its residence; and the great Elephant was found in ice.

It has been argued also, that, if there has not been an interchange of climate, there has been at least an universal diminution of temperature, because there are, in Europe, abundant fossil species, of which the parallels exist in tropical seas, but not in our own. The numerous species in the beds of Grignon, the melanopsis, nautilus, distichopora, encrinus, oliva, conus, tubipora, mitra, voluta, nerita, paludina, and melania, including nearly sixty species of marine and fresh waters, abound as fossil in Europe, but, as living, only in the tropical seas. But the value of this evidence is by no means plain. It is first a question of genera, not of species; as it ought to be, if it were to prove what is thought. There is a Coluber in Britain and there are many in hot climates. And no animals are so notedly indifferent to temperature as the mollusca. The very same species of helix are now found frozen in polar ice and broiling in the African desert. Thus much for all these evidences from the animals and plants themselves.

But the hypothesis having been adopted, it was also necessary to explain the causes: and having disposed of the *posteriori* evidence, I must therefore examine the *priori* one. There is no provision for interchange of climates in the present motions of the earth: the axis cannot thus have changed under the actual arrangement of the solar system. It is especially useless to assign so gratuitous a cause as the appulse of a comet, when the facts remain so much more than doubtful; and, if the argument from coal be admitted, nothing. If it be said that the earth was at first an irregular mass, and that it became spheroidal only through a long series of changes, chiefly through wear and renewal, and that thus any changes of axis and climate can be accounted for, the answer is the same. It is a pure hypothesis to account for facts which, themselves, do not exist.

Lastly, it has been said, in support of diminution of temperature, not of interchange of climates, by Buffon, De Luc, Breislak, and others, that this may have happened because the early earth, fluid from heat in the interior, was less deeply covered by rocks at its earlier periods, that much heat was extricated during the act of consolidation, or during the precipitation of rocks from water, or during that of the combination of their metallic bases with oxygen. Any of these things are possible; the general fact of a lowering of temperature, were it but through radiation, is probable: but of whatever other geological value such a supposition or fact may be, it will not bear on the present question, until the fact itself has been established as to living and fossil beings; which it has not. But as this is an interesting subject, which I cannot here discuss, I must refer, and chiefly to Breislak and Fourrier.

Of Colonies and of Transportations of organic Fossils.

These two circumstances respecting fossil remains, interest geology far more than much that has preceded. The former proves conditions of repose, and also periods of time; being especially important towards the proof of great duration in the intervals between successive revolutions. The latter is indispensable to many inquiries respecting the changes of the surface.

If it is not universally true that particular tribes of shells occur alone, it is a fact of sufficient frequency; as it is, that they lie undisturbed in the places where they died. This happens notedly in the ammonites, the cerithia, the nautilites, the nummulites, and others; some of which also produce masses of rock of great extent, even when of such microscopic minuteness that a grain weight would counterpoise almost fifty. The masses of rock thus formed entirely of shells in

Touraine, are well known for their bulk; as are those of Grignon, Issy, Passy, Chaumont, Villers Coterets, and other places. Immense beds are thus formed of Miliolites alone. Similar facts occur in Egypt and Syria, in Persia and India, and, in reality, in every country that has been accurately examined. If other shells intrude into such colonies, or if many are intermixed in one, the geological inferences are scarcely affected; since repose and length of time are equally proved. Ages must have passed before these enormous accumulations could have taken place; and we can sometimes form loose conjectures, at least, respecting this time, by attending to the increase of living beds of shell-fish and the deposits in lakes. If this fact proves repose, it would have been unnecessary to say that it is inconsistent with diluvian movements, had not the most recent of our observers asserted such motions even in the most decided cases of this nature. It is childish to repeat, that facts like this are irreconcileable to any precipitation of rocks from solution.

However true the transportation of fossil remains, through the action of water, may be, it has been often asserted to have happened where it never occurred, and has thus formed one of the most troublesome falsities in Geology; impeding the discovery of truth in almost as great a degree as any of the hypotheses which have been invented, and, above all, where such actions have been referred to the Mosaic deluge. And thus have other deluges been invented to explain what demanded no such solution.

To the remarks just made, proving that the living animals had been preserved where they died, I may add those facts which are often but repetitions of what has preceded. These are, the integrity of tender shells and the preservation of their minute parts, the preservation of plants, especially of leaves, incapable of bearing transportation, that of tender fishes, found entire, as in Monte Bolca, and the positions of the convexities of bivalves; all of them being evidences for any case, in practice, respecting which we may be called on to judge.

If, on the contrary, the reverse facts occur, marking motion, it is certain that no distant transportation is necessary for the effects, as there may often be fracture and dispersion of the objects, without any; while it is no less certain that such distant and violent ones as have been presumed, would have entirely destroyed such objects, to their disappearance. We know, through soundings, that recent shells are broken and intermixed in the exact manner which has, in fossil ones, been attributed to transportation; while the motions in these cases are but the daily actions of the waves. And these are the germs of future rocks, the types of past ones, as, at present, they are those of visible alluvia. Such is the difficulty of proving transportation in the ordinary cases of fossil remains. Nevertheless, it has occurred, demonstrably, yet to no great extent: but the important question here is, the geological purpose in view in these hypotheses as they have been constructed and used.

This is double. It is to explain simply the positions of organic fossils supposed to require explanation, but it is, far more injuriously to science, to attempt to prove deluges and other similar actions, through such facts. And this is the proper geological question, as it is the only one of moment. If I state examples of the principal cases, and their presumed causes, and also show that they can be explained by much simpler ones than have been assigned, the purpose here in view is answered. It is the explanation of admitted transportation, and the rejection of false hypotheses respecting the causes.

If the hypothesis of distant transportation has been applied to the case of Monte Bolca, it is already fully answered. Where transportation has been assigned as the cause of the positions of organic bodies which were assumed to have belonged to other climates, the answer to this also needs not be repeated. It is not a case of evidence as to the fact. I may say the same as to the fossil remains of Dalmatia and elsewhere: the fact of transportation is assumed; and the right solution of these cases will be examined in the next section. It has further been attempted to prove the transportation of the bones of land animals found in alluvia, by Deluges, from the presence of petrified remains in the same places. There are two cases of this; and such a theory is unnecessary for either. These marine remains are often the fragments of organic limestones, parts of the original alluvia in which the bones were deposited: it is a case of accidental contiguity, not of joint transportation. If it had been, the condition of both classes should have been similar. The other case is that of Italy, already explained: it is the occurrence of terrestrial alluvia above marine ones.

On the Siberian remains, I must dwell somewhat more: especially because this case has been made a foundation for a theory of the earth. These are often, though not always, transported, but under transportations of a far different nature from what has been asserted. Pallas informs us that the bones, skeletons, and entire carcasses, of the Elephant, Rhinoceros, and other large animals, occur near every great river of northern Asia, almost from the Black sea to that of Kamtschatka; yet being nearly limited to the alluvia of the plains. To conceive that a single torrent, or any succession of torrents capable of covering a breadth of four thousand miles, should have left these remains as they are now found, and so perfect, should have even left the carcasses as they are, is for those who see no difficulties when the inventions are their own. Such visions, warring against all common sense as well as philosophy, deserve no answer. How Pallas could have supposed them to have been thus transported from southern climates, he was himself bound to explain, since no one can do it for him. If De Luc's theory was intelligible to himself, I know not who else can understand it; though it did once meet a supporter who must at least have been inconsiderate in thus adopting it. This country consisted, De Luc says, of islands and seas intermixed, in a constant state of revolution of level and condition: and thus, in some manner, every thing is solved. Certainly, geologists have often deserved the ridicule which they have so frequently experienced. It is useless to inquire of the causes of such revolutions as this, when there is not a shadow of evidence as to the facts, and when it is not supported by any thing that we know of the earth. But it is especially unfortunate for this philosophy, that the elevated alluvia of Italy should have been explained in the same manner. It is difficult to conjecture what ideas are formed before such reasonings and conclusions. These are Werner's ever tractable oceans, under a new shape. He who has said that the very name Geology excited ridicule, should not have forgotten where the retort might fall with equal or greater justice, and that there were more than one fabricator of cabinet theories. The remark on this system would have been spared had it come from a less name: the mischief lies in this very thing, in the authority; which, ever injurious in proportion to its weight, is still weighty on this very point, injuring him whom it is the duty of a systematic writer to protect from false theories.

The animals in question died and were buried where they lived. If they are covered with beds of sand and clay, sometimes containing shells, this is what happens in every land of rivers; and as far as the bones themselves are transported, this is their only source of transportation. Natural actions or casual inundations account for every thing: but they account too simply: they are not Theories of the Earth. The shells are terrestrial, and they have been supposed marine: the perpetual error of those who philosophize in shells and thus determine Earths. Let there be marine alluvia any where, it is the case of deserted æstuaries. Nor is there any reason why the Lena or the Jenisei should not carry even colonies of skeletons to the sea itself, since this very event has happened in the Solway firth within our own knowledge.

Enough of these cases; they are examples to which all of which I read can be reduced. Of the causes of such imagined torrents, the following have been assigned; and I may here name what, for the most part, must be re-examined hereafter. The supposed eruption of the Black sea and of the presumed lake of Thessaly, are among these; and there are modern examples of such a fact, inferred in such a valley as Glen Roy, proved to the sight in Switzerland. Unquestionably, such accidents must have transported fossil remains, with all else; yet shells are little likely to have survived such movements as this. Let them have all their weight, and they may explain a few cases; but that is all. It is certain, reversely, that the buried remains are not competent to prove the causes, as also these apply to alluvial deposits only.

If such elevations of the earth as that of Italy be assigned as the cause of torrents of this extensive nature, which they unquestionably must have been, the general conclusion as to the fossil remains is the same. They are little likely to have survived; and they are not alone sufficient to prove such a fact. Italy has demanded a much more difficult proof, as I have shown, and far other reasonings than these superficial ones; else would it perhaps have been explained long ago. Of the Mosaic deluge, I might say the same, far more strongly. No transportation, be it ever so established, can prove that fact; and that fact itself, as detailed in Scripture, gives no reason to suppose that it was accompanied by such extraordinary torrents. But this is a question that must recur, mixed up as it has been with almost every thing in the history of the Earth. If the foregoing cases explain some of the sources of error among geologists in deciding respecting transported fossils, it is still necessary to point out others, and to explain some of those more fully: the actual cases of this kind, and their true nature, will thus develop themselves without difficulty.

There are two distinct circumstances, under which marine transportations have been imagined, where the fossils remain in their places. The retirement of the Caspian sea through the growth of the lateral alluvia is a case in point as to the first: the marine remains found now far beyond it, were once a part of its own shores. And all the great sandy deserts present the same false cases. Their waters are salt, because they consist of the red marl stratum, containing this substance; and therefore are their remains marine, thus far. These waters disappear daily, while the fossils continue in the dried lands: and thence the much wider and far more erroneous conclusion also, that the sea has retired in recent times from those tracts of sand.

The case of the more distant æstuaries of the present ocean is another: they have, equally, been mistaken for tertiary strata, and for collections of transported fossils, according to the circumstances; and these in particular, have repeatedly, and, now, very recently and perseveringly, been attributed to deluges, and to the Mosaic deluge. System will, in some, supply the place of ignorance in others: it is doubly unfortunate when system and bad observation are united. Though found more than a thousand miles from the sea, in India, these are but the tranquil deposits of the Indian ocean. Transportation there must be, if terrestrial and marine remains are intermixed: but this is nothing more than what I shall now explain, by what offers a solution for perhaps every case of such mixture yet described, whether in rocks or alluvia, antient or recent. And though the levels of such deposits should, in any case, exceed that of the present ocean, it would not alter these conclusions; since, as I shall hereafter show more fully, there is a vacillation of level between the sea and the land, in many parts of the world; and, even in our own island, the most undoubted proofs of the gradual elevation of the latter.

Of such transportations, even of land animals, I mentioned the case of the Solway firth. This occurred in 1794, when more than two thousand bodies of horses, cows, sheep, and smaller animals, were carried into it in one day. And it is a valuable fact, because it explains such cases as that of the Paris basin; and many more: it will be a future intermixture or alternation of marine and terrestrial remains, in shale and sandstone and limestone; a study for the geologists of other Earths.

On analogous grounds, may not only many of the cases of the tertiary deposits, but those of the Lias also be explained. Wherever this series presents such intermixtures, that point at least was an æstuary: the solution is simple, and it offers a perfect analogy to the æstuary of the Thames, or any other river, at present. The same mixtures from transportation are occurring there; and in future ages, or worlds, it may be a lias series, thus far. The alluvial antient æstuaries, just mentioned, are parallel cases under another form. And in actual æstuaries, as in more antient ones, marine and terrestrial remains, antient and modern ones, may often be confounded, since the proper marine bottom must be frequently mixed with the terrestrial deposits of the rivers, and in every manner. Cases of actual distant transportation, such as that of West Indian fruits to our own shores, seem too limited in quantity and possible effect, to demand any reference to antient deposits.

To guide the judgments of geologists on this subject, I must yet remark, that in such cases, errors may arise, first from mistaking the origin of the fossils themselves, and next from mistaking the nature of the strata; as it would be easy to point out recorded cases where both have been combined. And thus have such mistakes arisen, from determining the origin of the stratum through the presumed one of the fossil, or that of this last, from misapprehending the nature of the other.

In the chapter on the tertiary strata, I have already shown the difficulty of determining the marine or terrestrial origin of fossil fishes. As to the other error, all strata were formerly thought to be of marine origin: and thus were all fishes, in particular, formerly supposed to be of the same nature; whence, if found accompanying terrestrial remains, transportation was inferred, under either or both errors. And recent observations have not been free of the same mistakes and the same false inferences. The "bituminous marl slate" was an error of the former kind, of the Freyberg school: the supposed marine stratum is a fresh water one. At Glaris in Switzerland, ferns occur with fishes, and the latter must therefore belong to fresh water. Former observations had determined that marine and fresh water fishes existed with vegetables in the strata of Œningen, and thus were formed false and impossible theories respecting this deposit. If I am right respecting the versatility of fishes, the fact as to some of these species may still be true, or there may be marine fishes in these strata: the very Triglæ quoted may exist in it, though the specimens seemed to me more than doubtful; and yet this deposit is but a part of the antient bed of the lake of Constance. And this illustrates the other source of error.

I may therefore conclude. There is no proof of transported fossils, under any other circumstances, or from any other causes, than those which I have explained; and I hope that I have shown how these can be distinguished. But the distinction demands acute geographical observation, even more than geological knowledge; and here have observers especially failed. Of the Mosaic deluge in particular, I have no hesitation in saying, that it has never been proved to have produced a single existing appearance of any kind;

and that it ought to be struck out of the list of geological causes.

Of organic Fossils in Fissures and Caverns.

The fossils found in fissures belong strictly to geology, as they relate to somewhat antient alluvia; the proper geological relations of the latter occurrence are still very obscure, as this question also concerns zoology rather than the present science.

The former are involved in calcareous conglomerate rocks, forming portions of these; of which the cement consists of minute fragments and carbonat of lime: and they are therefore local alluvial rocks. If they sometimes occupy fissures, this is not always the case, as happens in Dalmatia; but the two forms need not be distinguished, especially as they all appear to have once occupied cavities of some kind. These rocks occur in Gibraltar, near Montpellier and Nice, in Arragon, Corsica, Dalmatia, Cerigo, and elsewhere. The remains are the bones of land animals; being terrene themselves, as are the deposits, and, except travertino, the only truly terrene organic rock. A few shells, doubtless terrene also, have sometimes occurred with them. The bones have appeared to belong to the Horse, Ox, Ass, and Sheep, to Antelopes, Mice, Birds, and some others which I need not here name. The geological question does not seem intricate.

The rocks are the accumulations of fragments which have been produced where they lie. Transportation is impossible under such circumstances; nor does the state of the fragments justify such a supposition. The production of the rocks themselves is easily understood. I have elsewhere described such veins of conglomerate in Scotland, in the older rocks, yet of evidently modern origin: we have only to inquire respecting the presence of the bones. In Guernsey there is a fissure in which a rock of this nature is now forming; and the animal remains are snails, with the skeletons of mice and weasels, united by calcareous matter. There are facts in natural history, which show that animals seek concealed places to die in, and that predatory animals conceal themselves in such retreats: the caverns to be immediately examined are striking examples of the same kind. If this be not the solution, I know of no rational one that can be offered. And the further geological conclusion is, that these rocks may be of any date posterior to the disclosure of the strata in which they occur, while that can only be conjectured through zoology, though seldom even thus. The organic rock of Guernsey, now forming, is in gneiss: the antiquity of a rock determines nothing as to that of a fissure.

And this is all: the question might be dismissed, if I were not compelled to notice theories which are still misleading geologists; as is my perpetually disagreeable duty. I have just shown it not to be true that they are not formed at the present day, as Cuvier has said: and there is not the least reason for concluding, as he has also done, that they must all be more modern than the antient alluvia containing animal remains. In individual comparisons, this may be the fact: the zoologist is generalizing from single instances in his own department, and in support of some hypothesis. They may be found as antient as the most antient organic alluvia: and this is probably the case with that of Corsica. And no geological relations can be effectually deduced from these bones, because, in almost all cases, this must be negative evidence. It has also been said, that the same bones were never found in these fissures and in the caverns also; because, in this too, there was, apparently, a zoological system to be supported. This, again, is generalizing without facts, and without correct reasoning. I have shown that this might happen, or ought to happen; and it has proved so, in the case of the horse, the ox, and others. And that I may dismiss the whole of these hypotheses at once, it is not true that the remains in the alluvia are not also found in caverns. The Elephant, Rhinoceros, Hippopotamus, Ox, and others, occur in both: so that whatever diluvian speculations were to be founded on this disproved statement, must be abandoned.

The Caverns and their remains have recently attracted so much popular attention, under that love of the marvellous which so often loses sight of science, and of truth also, that I can refer, without difficulty, to volumes in abundance, those who consider that all Geology is comprised in such a pursuit as this. A short statement would not satisfy them; and the concern of Geology with these facts is very small indeed.

They have been found in Germany and in England, in many places; near Gailenreuth, Bauman, Sharzfells, Kloeterhoele, Glucksbrun, and in other situations, in the former; and, in the latter, near Plymouth, Buringdon, Swansea, Llandebie, Torbay, at Kirkdale, and in other places, for all which Dr. Buckland's ample and minute works, especially, can be consulted. In these caverns, always open in some manner, (though it is plain that if filled more or less with earth, and broken into through the rock, that opening might not always be easily found,) the bones are more or less entangled in loose alluvial matter, generally of a fine texture; and also among calcareous stalactites. This is the sufficient geological description; unless I add that they prevail in the mountain limestone; very naturally, since this rock abounds in caverns.

But their chief interest is with zoology, existing, or extinct as to the countries in which they occur; since they have not yet produced animals demonstrably extinct as to the whole earth, though this has been said of certain bears. If I refer, as before, for the zoological particulars, I may name, without distinction of place, in Germany, the lion, tiger, dog or wolf, polecat, hyena, rhinoceros; and, in England, since the insular situation demands this distinction, the elephant, rhinoceros, hippopotamus, horse, ox, stag, hyena, bear, hog, fox, weasel, rabbit, water rat, mouse, raven, pigeon and more, together with human bones in two instances, the whole comprising animals, extinct as to Britain, and also existing.

The solution as to the simple presence of those remains is not very different from that of the former case. The caverns were the retreats, or the voluntary graves, of some, and in some of the cases; and, in others, the bones were deposited by the hyæna, in its residence, as is easily inferred from the circumstances attending this particular animal. And this, as a further question of zoology alone, is all that I need here say on that part of the subject in these cases.

In strictness, the case of these caverns has no other geological bearings, and, as to Geology therefore, it should be terminated; inasmuch as their interior alluvia must be explained on the same grounds as the exterior ones, as that will also be a future question in this work. But having already noticed the imaginary arguments for the Mosaic deluge, drawn from transportations as imaginary, I cannot avoid continuing the examination of them as derived from this case, especially as that record is ranked as a cause in geology.

Driven from one point to another since the time of Tertullian, as geological knowledge increased, those arguments have always sought refuge in the most recent unexplained appearance; and these caves are apparently the last hold of those who seem to forget that the truth of the Sacred Writings does not rest on physical proofs. The anxiety which is thus shown, to prove any of those records in this manner, is not a very politic mode of evincing confidence or faith in them: and if the arguments are worthless, the effect is evil, since, to have rested a proof on that which is not one, is to produce disbelief or doubt.

But, believing that record in its exact and literal sense, it is impossible to see in what manner it can be related to these appearances. In former hands, the remains in the exterior alluvia have proved nothing as to this record; it is not easy to see how the caves can prove more; and, unfortunately for the supporters of this hypothesis, they seem to prove less, in as far as the extinct animals are said to be antediluvian ones, since the caves contain existing, and therefore postdiluvian animals, and man among the rest, while his remains are said to be invariably recent. To admit what could not be denied, and still to maintain the hypothesis, is a species of philosophy which cannot well be discussed. If the one kind are postdiluvian, why are they not all of one period? But it is almost impossible to understand how these facts are brought to bear on this theory. The record says that all animals were preserved for the renovation of the earth after the deluge; and if any did arrive, why not all, or more? But it is better not to put more questions. This has been an injudicious interference at every period: it is even peculiarly imprudent in the present case, as the slightest consideration will show. The assertion that these things do prove the Deluge, may satisfy those who can thus be satisfied, by an assertion from authority: but the effect is not lasting, even among those: there is ever danger that conclusions the very reverse may be drawn by persons who know how to examine evidence. But I am glad to pass from a subject that ought never thus to have been mixed up with physical inquiries: more sorry that I cannot avoid recurring to it hereafter. There is abundant proof of a gradual change in the original balance of animals, all over the earth, and of gradual extinctions of individual kinds, in many countries. This is familiar of our own, in the Urus, Elk, Beaver, and Wolf, at least; and that which we know by record or clear evidence, has probably been going on from the beginning. There is no mystery here: had there been more, it would have been more acceptable.

And yet I cannot avoid noticing another of these speculations as to human remains. It is said to be a proof of the especially recent formation of man, that his remains are not found in the same alluvia as those of other animals. What support of Scripture is this? That record says that Man and animals were created within one short period. If they ought thus equally to be found, and are not, it is evidence against the record, not in favour of it. This is a strange oversight: but such it ever is with Systems, and especially where produced for a special purpose. But I desire to avoid all further examination of this disagreeable subject also: it is doubly painful, from the impropriety of these attempts to prove what rests on far different grounds, and from the names attached to these several speculations. Burnet has already remarked, and St. Augustin long before him, on the impropriety of attempting to prove Scripture by what may ultimately prove unfounded. That lesson was also urged by Bacon, it has often been repeated, but it seems to have been ever thrown away. Let us hope that we have now heard of this pernicious interference for the last time.

Of the Identification of Strata through organic Fossils.

Practical Geology is peculiarly interested in this question, and it especially requires to be investigated, not only because the utility has been highly overrated, but because it continues to be an impediment to geological knowledge, by supporting the hypothesis of universal formations, even when that is renounced in words. I have formerly shown, that in the manner in which strata were formed, their universal, or even wide correspondence was impossible: and if there is here a fundamental falsity, there is a still greater one, if possible, as to the animal fossils contained in them. The latter must be tried, as usual, by the history of the living earth. The same shell fishes do not live in the equatorial and polar seas, they are not even the same in the British channel and the Mediterranean: I have made the remark already, in speaking of the tertiary strata. This is fatal to such inferences: if those two seas do not contain identical races of living shells, whence should the limestones which we have thought fit to call Oolithes contain the same fossils in Italy and in England? similar laws as to former organic beings may be inferred; dissimilar or contradictory ones cannot.

Again, the strata now forming out of the Scottish primary mountains cannot be the same that are in the act of being produced by the calcareous lands of England. Yet both contain the same shell fishes. In a future world, Geologists who may reason in the same manner, will prove dissimilar strata to be the same, through similar fossils. There is no meaning in such philosophy as this: and that which is the history of the present alluvia is the history of the consolidated strata. There is no order of succession in living colonies, in modern seas; there cannot have been such an order in past ones, except under utter assumptions, and therefore strata ought not to be identifiable in this manner. And the sum of this *priori* reasoning, further is, that, to prove what is asserted, every species or genus should have succeeded in a definite order, and changed with each stratum: and this also, not within a limited space only, but over half or the whole of the world.

Every supposition necessary to this theory of identification is unfounded or improbable; and therefore such a test ought not to exist. It can be admitted within limited distances and deposits, such as England, or similar tracts, but not further; least of all, to the extent asserted. Nor does it exist as a fact: the experience confirms the *priori* reasoning. A limestone following the red marl occurs in England and France and Spain and Italy: it is the lias by position; if England is to be the standard for all Europe. It contains many different fossils in these several places; and the same is true of any other stratum or series. All this is well known to Geologists, yet they seem to persist in their system. Echini, turbines, tellinæ, and chamæ are found from primary slate up to chalk: they certainly do not indentify those strata. It is the same with madreporites, pentacrinites, entomolites, gryphites, pectines, anomiæ, ostreæ, patellæ, ammonites, and many more; and there are not even predominant associations of these. Conchologists themselves can extend this list, as I need not; any catalogue will enlarge it. Crabs exist as far asunder as the mountain limestone and the chalk. Crocodiles are found in the lias, the oolithe, the green sand, and the upper blue clay. Belemnites abound in the chalk of Ireland, and are rare in that of England. The exceptions are too abounding to permit of the rule; though I have given but a small number.

It is quite time that geologists should review their system, in this, as in far more, and correct their practice. Then, and not till then, shall we begin fairly to study the history of the Earth; and, in time, we shall know the Truth; the Truth itself, and the ever odious truth that we have been in the wrong. Let shells be used as aids, as far as they can: but if strata are to be determined by shells alone, it will be the land of shells and conchologists that will set the pattern of the earth, as other lands and other conchologists, will, under Breislak's remark, contest for other patterns. This is not geology; and thus will Geology never be.

I terminate this chapter by referring to the catalogues of conchologists as connected with the strata, whether in this or in other countries. They will furnish many facts of reference for much that has preceded: but I could not, with any propriety, encumber this work with them. The geological bearings of these most important bodies in the history of the Earth have been stated as far as I was enabled to do so from the information in existence.

CHAP. XXI.

On the successive Forms of the Earth: Revolutions of the Globe.

The subject of the present chapter is intimately connected with a rational theory of the earth. It comprises the history of those revolutions of the surface, previously to its last, or present, condition, which have, at different times, involved the destruction of some, at least, of the organized beings by which it was inhabited. To these revolutions we are indebted for its most striking and important features; as to these it is owing, that the animals of former days are now known to us, and that their vegetables are treasured up for the supply of our wants. If the magnitude of the powers which these changes involve, and the undefined ages which they demand, are alarming to those whose views have been confined, by timidity or prejudice, to a narrow circle of obvious facts, let them recollect that nature every where displays the marks of enormous power more than once exerted; and that in the destruction which she every where exhibits, in the equally extensive and tedious successive loss and renewal of races of organized beings, and in many other geological phenomena which I need not now enumerate, there is implied the necessity of a duration to which we dare not assign a boundary.

In preceding chapters, the nature and the appearances of strata and of stratification, the changes of position which these have undergone, and the sources of the unstratified rocks, have been so far explained, as to enable the reader to follow without difficulty, the views which it is intended here to lay before him. In a certain sense, these form a summary of some parts of this work. The subject indeed still abounds with difficulties which I do not pretend to remove; but if the facts which bear on this important train of events shall here be arranged in a manner somewhat more comprehensive, this chapter will have fulfilled all that it proposes to execute.

On the Evidences of Revolutions in the Globe.

The marks of great changes which the earth contains, consisting in the successive deposition of new strata, under circumstances that prove the existence of intermediate intervals, imply successive periods of repose, interrupted by revolutions indicating the exertion of enormous forces. During repose, the strata were formed; in the intervals, they were displaced. If any of these revolutions can be shown to be universal, it will follow that the whole surface of the earth has been changed once or more: if, on the contrary, they are only partial, it cannot be concluded that the changes, or actions, on which they depended, however extensive, were of a nature to involve the entire globe. The evidences for these revolutions and alterations, must be sought in the changes or reversals of position exhibited by approximate beds, in that part of the composition of strata which indicates the existence of previous rocks, and in the presence and position of imbedded organic fossils.

From the mode in which alluvial materials of different kinds are now deposited beneath water, and from the general doctrines of stratification formerly examined, it has here been concluded, that a series of strata consecutively parallel, using that term in a geological, not in a mathematical sense, has been generated during one period of repose in the form and place of the waters under which it was produced. Where, on the contrary, the positions of approximate strata cease to be parallel, or where they must be considered as reversed, in a geological sense, it has also been inferred that an interval, or interruption, of greater or less duration, took place between the deposition of the inferior and that of the superior series; however impossible it may be to conjecture its extent, or to prove the exact nature of the interruption. In most cases, at least, it is further concluded, from appearances which I need not now repeat, that the inferior series was indurated to the state of a rock, before the deposition of the loose materials, destined to be similarly indurated at a subsequent period, commenced. It is further evident, that as the inferior strata, like the superior, must have been deposited in a position, horizontal or nearly so, their present state must have been the result of some disturbing force: and if, to simplify this general view, it is assumed that the upper series is horizontal, and the inferior elevated to some considerable angle, the same reasoning will apply, under the necessary modifications, to cases where the inferior strata are not at a high angle and the superior not at a low one; because the first change of position, which involved only the inferior strata, may have been followed by others which affected both; a supposition proved by a variety of facts already mentioned, which will necessarily be again noticed here.

Thus, an interval of time, including a disturbance, is proved by every reversal of the positions of approximate strata; as an interval of repose, apparently much longer, of the surface of the earth, is also proved by the consecutive arrangement of any series: while the extent of the series in question thus also gives an approximation to the length of that period of rest. This rule however is subject to exceptions: but they are such as do not vitiate its value. As formerly shown, from the mode in which elevated strata lie, those which succeed to them must often be parallel or consecutive, as well as reverse; and this double relation may even take place within very small spaces. In such cases, the interruption is determined by any tract, however small, of a reversed position; or by other parts of the relative characters of the two approximate series, on which it would now be superfluous to enlarge.

Now although these reversals of position indicate a revolution at that particular spot, they do not prove that such changes were universal or extensive. The extent of these is to be judged of by other circumstances and principally by the extensive range occupied by the inferior and by the superior strata; as will be more fully shown, when the number and nature of these probable revolutions shall come under review.

If there are many cases of revolution which can thus be proved by a comparison of the positions of approximate strata alone, there is one, at least, for which we must resort solely to other evidence. In many instances, this evidence is interesting, even where it may be superfluous; while, in some, it serves to indicate that which mere change of position does not, namely the magnitude and extent of the revolution, and, that within undefineable limits, a long interval of time had elapsed between the termination of the inferior and the generation of the superior series.

This evidence consists in the fragments of previous rocks imbedded in the later strata, or in the nature and composition of the conglomerates. By means of these also, we can sometimes trace, though in an imperfect and general manner only, the sources whence the newer strata were derived; and thus they also afford some light towards determining the question of locality or limit, with respect to particular deposits. The mode in which these rocks are calculated to afford this evidence ought already to be apparent. The local conglomerates, formerly distinguished from the general ones, do not enter into this question, as they have originated in local causes of another kind. The trap conglomerates are equally excepted, as depending on revolutions in which this family of rocks is separately engaged. But the general ones, consisting of the fragments of different rocks, agglutinated by materials of a finer kind, are formed out of the alluvia which, in a former state of the surface, were the produce of these. As the fragments of the present strata, produced by the action of the wasting forces, are now forming beds in such places as are fitted to receive them, so are these destined to produce future conglomerates, should other revolutions, similar to the past, occur. And as these accumulations necessarily bespeak a long period of time, so do those which enter into the conglomerate rocks equally indicate a long interval, during which the substances out of which they are formed, occupied a place above the waters or constituted a dry land marked by hill and valley, and subjected, as the present is, to the action of rivers. The great depth of the finer strata which commonly follow them, equally prove the length of such intervals of repose, before they were again lifted above the waters by subsequent revolutions; but they do not prove in the same decisive manner, that the rocks from which we still suppose them to have been produced, were actually indurated. Neither is the motion or the transportation of the materials so well indicated in the finer strata; while the fragments found in the conglomerates, often tend to illustrate other circumstances of importance, which could not be proved by the examination of those.

The presence and position of organic fossils, are circumstances of a limited, though certain value, in proving revolutions on the surface of the globe; though this species of evidence is sometimes rendered nugatory, by the multitude of concomitant circumstances which affect these bodies, and by the doubt in which we must consequently remain respecting their successions or interruptions. It is true, that some are found in the primary rocks, elevated high above the level of the present ocean. It is equally true that they are found similarly imbedded in the secondary strata, as well as in some deposits, subsequent to these, or independent of them. It has also been shown that certain strata do not contain them, that the antient are less numerous than the modern, that they differ in some degree as to the species, and that many appear to be extinct. But none of these facts are sufficient to prove, absolutely, that there has been an interruption in the races of these beings; or that an antient set of inhabitants was extinguished before a subsequent one was formed. Unless that could be proved, they offer no further testimony, in most cases, on the subject of revolutions, than the conglomerates do, although they are valuable records of time, as formerly remarked: and that this cannot be proved, I have shown in the preceding chapter. But there is one case in which the evidence afforded by organic fossils is peculiarly valuable. It is that of the coal strata; in which, the presence of terrestrial bodies proves the production, above the level of the sea, of strata which, but for that evidence, we might have supposed to have been formed below it. The full value of this fact will appear when these strata shall hereafter be examined.

In proceeding to enquire into the number of the revolutions of the surface of the globe which appear capable of proof from the evidence thus indicated, I must remark, that it will depend on considerations of other kinds, what the extent of these has been; how far they may be considered local, or general, or universal. It is also scarcely within the limits of our means, to enquire into the lengths of the intervals of repose between successive revolutions; since the evidence on which this ought to rest is of a very imperfect nature, from the numerous accessary circumstances by which it may be vitiated: though the little which may be fairly offered in the way of rational conjecture, will be stated. And the reader is now prepared to understand the causes of these revolutions; as they have already been examined in treating of those elevations of strata which offer the most convincing proofs of them.

There was a time when Geologists expected that every enquiry respecting the structure of the earth should contain an investigation of its origin and creation. That period, it must be hoped, is past. If there are any who are still desirous to commence their researches from periods which lie beyond the scope of observation and reasoning, they can seek in the works of the Cosmogonists. Where we have neither experience nor analogy for our guides, all is conjecture and darkness. As far as these extend, we are within the bounds of philosophical inquiry: beyond it, lies the free space, the ethereal region of poetry.

I must nevertheless allude, at least, to the speculations of La Place, resting on far other grounds than those of the Cosmogonists in question; though I presume that they are too well known to require detail. They are rational and philosophical, should they not be proved true; but I do not feel that I am even justified in stating the appearances and analogies on which they rest, in a place where I have undertaken to detail only what can be deduced from observation and reasoning upon the earth as we know it. Hereafter, this fundamental Theory of the Earth may find a fitter place.

It is the object, therefore, of the following discussion, to trace the disposition of the rocky surface of the globe, from that most distant or early point at which the marks of change are perceptible, and to pursue its changes down to the present day, from the time at which these marks of revolution disappear. Beyond that distant point, it is possible that there may have been other changes; but, of these, we can find no Evidence. A curtain is here drawn, to separate the visible world from that which is, to us, as if it had never existed. That this System had a beginning, we are certain: where that may lie, we know not; but, for us, it is placed beyond that æra at which we can no longer trace the marks of a change of order, of the destruction and renovation of its form. It is from this point that a Theory of the Earth must at present commence: it is from this also that the present inquiry begins. But it forms only a portion of what belongs to a Theory of the Earth. It is an attempt to detail and establish but one train of facts, yet of important ones, and to indicate the nature of the evidence and the reasoning by which they are supported.

On the Revolutions concerned in the primary Strata.

I have formerly shown, that whatever differences of antiquity there may be among rocks, we can only prove those relative periods, satisfactorily, in the stratified ones, under some exceptions which I need not now repeat; and to these therefore must this inquiry be confined. Whatever relations these may bear to the horizontal plane, their geological inferiority or superiority is unaffected by those, and is assignable: and it is in the lowest, therefore, under this sense, that we must seek the first or original rocks: not the "primordial" ones of theorists, but the first respecting which we can procure unquestionable evidence. And we know that they are prior to any unstratified rock, because it is the production of these which has changed the positions of the others. The latter, taking them as one mass, form the primary strata of the present arrangement of geologists, as the lowest of this set is the oldest stratum; while the consecutive general parallelism of the whole, proves that they have been formed during one interval of repose in that ocean under which their materials were accumulated.

If now there were no other evidence than that of position, for the antiquity of a rock, these primary strata, generally considered, would be the most antient; and the lowest of them, supposing this ascertained, the first of all. But I have shown that there is another kind of evidence, to be derived from their structure, or ingredients; and, by reasoning from this, we can safely infer that there has been at least one order of rocks, prior, even to the "primary;" whether more than one, will be immediately seen. Many of the primary strata contain fragments of older rocks, besides the finer materials; as I have fully shown, in the history of individual strata. Micaceous schist thus includes fragments of limestone, quartz rock, hornblende schist, granite, venous quartz, and of other micaceous schist: and thus also does quartz rock contain pebbles of quartz, with fragments of jasper, micaceous schist, and clay slate. In the graywackes, or compound varieties of the argillaceous schists, the same is notorious; but the evidence is complete without this. Thus it is proved, that, prior to our primary strata, there have existed, at least, granite, micaceous schist, hornblende schist, clay slate, jasper, limestone, quartz rock, and quartz occupying veins. There may have been others; but, in the mean time, it is important to observe the exact resemblance between the present primary rocks, and the still more antient ones, from the ruins of which they have been, partly at least, formed.

Now as the compounded rocks now forming are produced by the consolidation of materials carried from the land into the sea, it follows, that before the formation of the present primary strata and while they were still buried beneath the water in their germs, there was a terraqueous globe; an earth containing land and water, mountains, rivers, and seas. That earth also was formed of rocks similar to those of the present primary strata, and further, it is important to observe, of granite also; proving that this agent had then, as in later times, been the cause of the elevation of the strata. That the sea of this globe was inhabited by animals, is proved by the presence of organic fossils in the primary strata, as described in the last chapter; but if there is no proof that the land was also thus occupied, I have shown in that chapter, that the reverse cannot be inferred. If the anthracite of which I there spoke be admitted to be of vegetable origin, there is proof of what were probably terrestrial plants: and if, as is said, vegetable fossils have been found in the primary argillaceous schists, this proof is unquestionable. We have, at least, no right whatever to assume that it was not an inhabited earth in every mode.

I may now inquire whether, by the same mode of investigation, we can trace a still anterior globe. This, it is plain, can no longer be done by the examination of the nature of whole strata, but it may be attempted by that of their fragments. It might indeed be determined by the fragments of clay slate alone which enter into the composition of the primary strata. We are certain, from the imbedded fossils, that our own primary slates were formed out of mud deposited in the sea, and the produce of rocks once existing above the surface. I may extend the same reasoning to those rocks antecedent to the primary strata, which, once elevated above the earth, furnished the fragments in question. These then were the produce of a former sea, the receptacle of the ruins of rocks still more antient, and similarly elevated above the waters to which their waste was conveyed. There is no reason for limiting the application of this argument; for it applies equally to each preceding set of similar phenomena, as it does to the latest, which we can almost be said to witness in the present deposits of clay. But as no one deserving the name of a geologist can doubt that the imbedded fragments in the older strata are really the fragments of prior rocks, whatever may have been said by certain writers, no difficulty can arise on this subject. Hence then I have drawn the further conclusion, that there was one terraqueous globe, one earth divided into sea and land, even prior to that last named; containing mountains to furnish, and an ocean to receive those materials which formed the second set of mountains whose fragments are now imbedded in our primary strata, or in those of a third order. Geologists may perhaps be startled at conclusions which they have hitherto overlooked, obvious as they are, and clear as the reasoning is: how they should not have been seen by those who have shown such anxiety to maintain the antiquity of the globe, it is not for me to explain.

Thus I have traced a world, the fourth at least in order backwards from the present: how much more distant from this, I shall inquire hereafter, that I may give the reader a resting place for that which requires reflection. But at this point all evidence fails. Beyond it we cannot now go; and, beyond it, can perhaps never hope to ascend. Fragments may indeed be imagined, imbedded in our primary strata, of so complicated a character as to be capable of extending this evidence even one stage further; but it is scarcely to be expected that any thing of this kind should have been preserved through such a series of destruction and renovation.

Whether that most antient of all the Earths, which is thus marked out by these delicate, yet unquestionable evidences, contained animals, we cannot determine. It is a question that would be decided in the affirmative, if organic remains were to be found in the fragments which enter into the primary strata. We cannot hope for such evidence as this; but, from its absence, we have no right to determine on the negative. It is something, to have traced even an inanimate globe through such a series of changes; to have arrived at that which is, to us, the commencement of all order. On what existed before that period, we may form conjectures, but we can reason no longer: they who choose, may imagine even prior revolutions; since it might be supposed that the strata of the most distant earth, thus pointed out, must have been produced from a still prior similar one. But where evidence ceases, the limits of sound philosophy are drawn. The last globe that we can trace, is, to us, the primordial one. Beyond, is the region of hypothesis: it is for this to speculate on what preceded, and to fix the period of absolute Creation.

With respect to the nature and causes of the revolutions belonging to these remote conditions of the globe, we can but argue from general analogy, and thus presume that they resembled the later ones respecting which we possess evidence. Yet we are not absolutely deprived of some evidence as to the causes; since the fragments of granite in the micaceous schist are sufficient to prove that the strata immediately anterior to our primary ones had been elevated by this substance, just as these have been.

It was necessary to commence from the primary strata, and to conduct this enquiry retrogressively; because the only evidence of the earliest conditions of the earth consists in their constitution. But I must return to the same point, for the purpose of tracing forwards, or to later periods, the revolutions succeeding to that which first elevated the primary strata from the bottom of the ocean. It must be remembered however, that the term primary is here used in its accepted sense, as a conventional distinction, adopted for the purposes of an antient artificial classification.

In this state of the earth, the present primary strata occupied horizontal positions beneath this ocean; though we are uncertain whether certain parts of those which we now esteem such, might not have been the very mountains whence they were formed. This is probably the fact; however incapable we yet are of proving it, owing to our imperfect observations, and the still more imperfect views which geologists have hitherto taken of a theory of the earth. We cannot conceive that all the supramarine land which produced the primary strata should have been mouldered and transferred to the sea before these underwent their first disturbance, nor that it should all have been depressed beneath the sea while the new-formed rocks were elevated.

Whatever may be judged as to this, the present relative position of the lowest secondary strata to the primary, shows that the latter must have been displaced before the deposition of those, and that, here, a general revolution has occurred. And it is also plain, that by this revolution, one portion of the primary strata must have remained beneath the waters, to receive those deposits which produced the secondary; while another must have been elevated above the ocean, to furnish their materials. It might indeed be imagined, that a continuation of the same actions on the mountains which furnished the materials of the primary strata, had furnished those of the immediate secondary also, and that no portion of these had been elevated above the sea in this revolution, however displaced beneath it. But this question is answered, by showing that the lowest secondary stratum contains fragments of rocks which can scarcely have been derived from any other system of strata than that of the primary ones.

I must now remark of the "transition" rocks of Geologists, that as this term was founded on a false and impossible theory of the deposition of rocks, and as the nature and boundaries of this class have never yet been defined or proved, I have not given it a place in this work, among the facts of Geology. Yet if it should really be established by future and better observations, I shall owe the science an apology to which I cannot yet consider any geologist entitled; and the views just held out must be modified accordingly, by the addition of another condition of the earth and another revolution. On the probable nature of this, it would be idle to speculate until proofs of the existence of such a series, distinct from the primary, and intermediate between it and the lowest of the secondary, shall be produced, and till it shall be shown what relative position it bears to the former.

We have now therefore a new ocean and a new earth; a state in which we are sure that there were sea and land; an ocean receiving the ruins which form the lowest of the secondary strata, brought into it by the flow of the rivers, and mountains, from which these rivers flowed; consisting of the very rocks which form the primary mountains of the present globe. In that new ocean lived the animals which are now imbedded in the most antient of the secondary strata; but our proofs are imperfect when we try to discover whether that dry land was also inhabited. No remains of land animals are found in the lowest red sandstone, or in the mountain limestone which succeeds it; but the same evidences of a vegetable world exist, as in the case of the primary strata. Yet this absence does not prove a negative. These strata were formed beneath an ocean; and, under that, terrestrial plants or animals could not have been deposited, without a transportation which few could have resisted with the preservation of their integrity; which none might. It is still possible that such remains may be found, as our experience is yet limited as to these subjects.

Whether the revolution which first elevated the primary strata from the bottom of the sea, was universal, or partial, we cannot determine. Theorists have considered it universal, it is true; but the proofs are far from conclusive. These rocks are indeed found so widely disseminated over the present surface of the earth, that, in a certain sense, they may be called universal. But it must be remembered that they do not appear above that highly disproportionate extent occupied by the ocean; so that even if proved to be elevated where they are now visible, at one period of time, that revolution would not be universal in the strict sense of the term. But perhaps this is to refine too much: it would be fairly judged so, if it were not proved that many revolutions of the surface were strictly partial, and that every one ought therefore, probably, to be viewed as involving changes of greater or less extent.

It must here be recollected, that the whole of the primary strata as we know them, were not brought to their present places by one revolution; we are certain that they underwent two, and even more; as it is evident that they must have been elevated a second time, at least, together with the secondary strata, were there even but one revolution as to these. The rocks which furnished the materials of the latter were unquestionably elevated above the ocean by the first revolution which displaced them; but as they were again elevated a second time, or oftener, together with the secondary strata, other rocks, or fresh portions, must have emerged at the same periods. And all these must have emerged for the first time, which are now covered by the secondary strata; though they had previously been displaced beneath the sea, since these strata were deposited on them beneath the waters. If as has been supposed, the present secondary strata did once entirely cover the primary, the whole of these latter must have remained at the bottom of the ocean, after their displacement; and no dry land, consisting purely of the primary rocks, ever existed. But we are sure of the contrary; because it is only in such a situation that they could have furnished the materials of the secondary. Hence therefore I must conclude, that some of the primary strata never were covered by any secondary ones; while the others have emerged during that subsequent change which brought these to the surface. In some places, these have probably been laid bare by the actions of waste now going on; thus preventing us from distinguishing the two cases: in others they continue covered. To assume, as has been done, that the whole of the primary strata were once covered by the secondary, we must conceive, that, after one elevation, they were again entirely depressed beneath the sea for the purpose of receiving these; while, even then, their production cannot be explained, as there could have been no supramarine land. This is to invent an impossible hypothesis to account for that which cannot be proved to have existed.

Having thus examined the successive conditions of the globe that were anterior to the appearance of the secondary strata above the ocean, it is proper to enquire of the great question of Time. It is not, of course, meant to ask what was the absolute length of time which passed from the first condition to that last examined, nor of that which was appropriated to each particular condition. Such questions must for ever lie beyond the sphere of our powers. But we can discover that the whole period must have been incalculably great, that even one of the portions was of immense duration; and hereafter, in comparing all these portions with each other, up to the present time, although we shall never discover the absolute length of any interval, we shall be able to draw inferences respecting their comparative quantities and general sum.

We have, in the first place, no means whatever of conjecturing the length of time occupied, in any case, by the interval of change or revolution. Whether the elevation of the strata which accompanied and immediately produced it, was of long or short duration, whether it consisted in a series of repeated actions, or in one sudden and violent catastrophe, must for ever remain unknown to us; though the present nature of volcanic action, to which it must have been analogous, should lead us to infer a considerable duration, and repeated actions. But we are not left equally in the dark respecting the interval of repose; that interval during which there existed a world divided into sea and land, and, in one case, at least, of the different periods up to the point in question, inhabited. That interval is, in every case, measured by the submarine deposits of the sea which was the ocean of that world; and to these we gain access in the subsequent revolution which brings them to the surface of the earth. We have every reason to know, from what is now taking place on our own earth, that the accumulation of materials at the bottom of the ocean, is a work infinitely slow: we are sure that such an accumulation as should produce the primary strata as we now see them, must have occupied a space, from the contemplation of which the mind shrinks. Whatever that may be, the geological depth of the consecutive series of any one stage of the surface, is the measure of the time through which it was deposited: it is the measure of the duration of that world which immediately preceded the one of which it forms the latest stratified portion.

Hence the duration of that state of the globe in which its dry land consisted of the present primary rocks alone, is measured by the nearest consecutive mass of the present secondary strata; and as we have not yet examined into these, this question cannot be investigated till at a future period of the present enquiry. But, for the same reason, we are competent now to enquire of the duration of that world which preceded the primary, because it is measured by those strata, now, equally with the secondary, subject to our inspection. The thickness of these strata we know to be enormous, although subject to great variety. But, in this variety of depth, the present question of duration is not interested; since that must be measured by the greatest, not by the least. How these depths are discovered by geological observations and inferences, was shown in a former chapter; and that they extend to many miles was also proved. Their absolute depth is not interesting in this enquiry, because we know not, in any case, the relation between that and the time required to produce it. It is here sufficient to show how the immensity of the one is implied by the magnitude of the other. Yet it is also interesting to observe comparatively, that as the greatest depth of the primary strata yet examined appears far greater than that of the first consecutive portion of the secondary, so the duration of that state of the earth which preceded the one in which the present primary rocks formed the only surface, was greater than the duration of that which immediately followed it, and of which the length is measured by the first portion of the secondary strata. The primary strata measure the repose of the preceding ocean.

Beyond this we cannot go a step on the subject of duration; at every point anterior to it, all must for ever remain unknown, because we have no longer any deposits of strata by which to calculate. Changes have been proved; and, with these, our enquiries must inevitably cease. Here also I must terminate the history of those states of the earth which bring us down to the secondary strata; to that which has been supposed the last great revolution of the earth, but which, I hope to show, is only one out of many, of which the traces are legibly written in characters that cannot be misapprehended.

On the Revolutions concerned in the Secondary Strata.

While the present primary rocks formed the sole dry land of the terraqueous globe last described, the usual causes of waste were depositing on the bottom of its ocean, materials destined to rise again in future strata. As the former had been already displaced, the horizontal deposits which thus covered them became necessarily inconsecutive; preserving, however, a parallelism in some places, while they assumed, in others, one variously reverse, according to the previous state of the foundation on which they were laid. In that various position, we now find them elevated above the surface of the water; and thus the secondary strata, or that division of them now under review, are distinguished from the primary, though they should not every where be inconsecutive. It is sufficient if that occurs in any one instance; because, had they been formed after the primary, in the same period of repose under the same ocean, it could not have happened, even in one.

I need but repeat now, that the primary strata furnished the materials of the secondary ones, being thus, above the ocean; yet this fact explains another set of geological appearances, of some importance, not yet noticed. This is, the absence of the lower order, as of any other superior ones, among the secondary strata, even when still higher ones are present. The primary ones which had emerged, and could not therefore have been covered by them, became depressed by subsequent revolutions, as I shall hereafter show, and thus received the deposits of a later date, which might thus have been stratified in contact with them, even as far up as the uppermost; while I have formerly shown that this occurrence is frequent, under many modes. And as it will be unnecessary hereafter to point out how this might have occurred in specific cases, in the several portions of the general series, I shall not repeat the remark. Among the complicated elevations which have taken place, it is easy to find solutions for every such instance of intermediate deficiencies: either in this cause, or in the removal of strata which had once been deposited, the foundations of which had been afterwards submerged to receive new ones.

I must now enquire respecting the period of repose which took place in that world, of which the primary strata formed the dry land. Of that interval, the highest limit is the period of the elevation of these rocks from the sea, or that of the commencement of the deposition of the secondary strata; and the lowest is that at which this, the next subsequent series, or any part of it, was elevated. It is therefore necessary to ascertain the place of the next revolution which followed the one which raised the primary strata; and this can be done, only by examining the whole of the secondary, and by observing where the first indications of such a change occur. That it did not involve all of these, as denoted by this general term, I hinted before, and shall now attempt to prove; by showing that the greater part of them were not in existence at the time of the first elevation.

We do not know the nature and number of the unconformable strata which follow the primary in every part of the world; while I have also just shown, as I have formerly proved by facts, that deficiencies may occur, in such a manner as to permit, in one place, an upper member or an upper series to come into contact with a rock, from which, in some other, it is separated by a great number of intervening strata. Thus, even strata high up in the secondary series have been shown to come into contact with gneiss and with granite. This fact therefore does not affect the general reasonings that are to follow: in some cases, it may show that certain revolutions were partial: in others, it is explained by various circumstances already examined, such as the loss of pre-existing strata, and the effects of the elevating and disturbing forces. It is sufficient, for the present purpose, to assume the lowest secondary strata which exist in any part of the world. If these should prove very limited, it will follow that the revolution which disturbed them was partial; if widely diffused, that it was more general, or perhaps universal. To simplify this question, therefore, I shall take the lowest of the English series, the red sandstone and the mountain limestone, both of them inferior to the coal strata. Whatever rocks in any other part of the earth shall be found to hold an analogous position, be their qualities what they may, they will be included in the same general reasonings; care being taken not to decide in these by their inferior, but by their superior portions; not by the rocks on which they repose, but by those which follow them in the order upwards.

The lowest secondary strata then, in England, are followed by a series of sandstones and shales interstratified with coal; and at that point the first revolution or elevation of the secondary strata must be fixed. If such an interval of revolution cannot here be always proved by the two first classes of evidence originally laid down, namely, contrariety of position and the presence of materials derived from the rocks immediately preceding, it is amply evinced by the third, or by the presence of terrestrial remains. The coal strata were formed above the level of the sea; and, being founded on the inferior secondary strata, it is evident that these had become, to a certain degree, dry land before that period. This evidence indeed is abundantly confirmed by the occasional discordance of position between the two series.

Thus then we have proofs of a revolution, partial or otherwise, which elevated the inferior secondary strata; and it is the third, at least, in order, of those which have occurred. To conjecture its extent, we ought to know, over how many parts of the earth these inferior strata can be traced. On this subject, unfortunately, our information is still defective; from the confusion which some geologists have made respecting the secondary strata. But if the coal series of England should be found widely diffused over the globe, it would prove that this revolution had been, at least equally extensive.

It must now however be observed, as before hinted, that the secondary strata could not have been elevated above the level of the waters without their foundations; and thus therefore the primary strata are proved to have experienced two disturbances, at least, up to the present point: two, if these revolutions were general; many, respecting which we can form no conjecture, if they were partial and successive, or different in different places. And as the period of repose of that world in which these lowest secondary strata were formed, is measured by their depth, it was apparently inferior in duration to that of the condition which preceded it; as their greatest or average dimensions do not appear to equal those of the primary.

At this point in the revolutions of the globe, there commences a very intricate set of changes, without which it is impossible to explain the generation and position of the coal strata. These occupy detached places, reposing on the inferior rocks wherever they happen to lie; often, if not always, in an unconformable position to those, and, frequently, very much disturbed. They consist of various alternations of sandstone, shale, clay, and coal, and occasionally of limestone; varying, almost every where, in number, in order of alternation, in the proportions of the several substances, and in the total depth of the whole deposits. We have no reason to think that they are the produce of different æras, if they do not always lie on the same rocks; for the reasons already stated: there is, on the contrary, some reason to think, but no proof, that they are the produce of the same; because of their general similarity of character, because of the nature and extent of the strata which immediately follow them, because of their correspondence in geological features, and because, to suppose otherwise, is to render still more complicated the view of the revolutions on which their present states must have depended. I must at least reason on them as if they were of one period; that I may preserve the needful simplicity of this enquiry: should they prove of different æras, the modifications that will be required are obvious, and the train of reasoning is pointed out.

The composition and nature of these strata, no less than the presence of their peculiar organic fossils, prove that they were formed, as far as they are earthy substances, from the ruins of rocks then above the surface of the water which received these. Those must have consisted both of the primary and the lower secondary strata, as both were then elevated above the waters. But although the coal strata were formed beneath water, they could not have been produced beneath the ocean. They include, throughout, the remains of terrestrial plants which could not have undergone transportation, as the very coal itself is proved to be the result of an accumulation of vegetables. Their analogy to the recent depositions of peat, and to its alternations with sand, marl, and clay, as these occur in our present fresh water lakes and bogs, equally tend to show that they were formed on the land; in marshes, or on the margins and bottoms of lakes and æstuaries, in a manner similar to that in which strata are now formed in lakes or beneath the ocean. Though there should be some difficulty in accounting for the great depth and the numerous alternations in some of these deposits, the general principle is not affected. If, during the present state of the surface, several alternations of peat and earthy matters can be formed in our lakes and æstuaries, much more numerous ones might have been produced in antient periods, under greater duration, and under variations of the state of the surface, the chief of which, as bearing on this question, I shall shortly explain.

Here then is a state of the earth, during which a large portion of it, laid dry, contained cavities, lakes or basins or æstuaries, in which were preparing a peculiar set of strata; while that period of repose is measured by the depth of the coal series in any place: and as we know that in some, it is very considerable, the period in question must have been correspondently great. But while these deposits were forming above the sea, it is plain that others must have been also forming below it; and the depth of those will correspond, under differences which we shall never know, with those of the coal strata; being formed during the same period of repose. This could not have been otherwise; and we must therefore expect to find, that as the coal strata are not every where intermediate between the inferior set and that which follows them, some other strata must, in particular places, be found occupying that space. Such marine strata must be parallel in time, but not in order, with those of the coal series; although, having been afterwards subjected to the same revolutions, they must correspond with them in their new positions or elevations. Thus then the strata of this epocha must present complicated appearances: seeming to have been formed at different periods, when they have been simultaneous, and the differences depending on those of their receptacles.

Geologists have not yet produced satisfactory descriptions of a state of things which seems nevertheless unavoidable. In fact, having never reasoned correctly respecting the coal strata, they have had no guide to a correct system of observations; while assuming a false theory, or rather, omitting to form a right one, they have viewed these as if they formed part of a general series produced under the ocean; thus giving them an intermediate place between the inferior ones and those which were, in some cases, superior, but could not have been so for the whole series upwards from the strata below the coal. At present, I know not how to correct this error, or fill this blank, from such observations as I have been able to make, or by comparing the reports of other observers; since it is always extremely difficult to reason from bad observations, when truth, and not system, is the real object. But it must be plain, that we are in great error or ignorance as to the order of the strata from the lowest red sandstone and its " mountain" limestone upwards, that the confusion or ignorance depends on this very fact, so utterly overlooked, and that till this portion of the later rocks is investigated and settled, there will remain a serious blank in a most essential portion of Geology. At present, I can do no more than point out the principles of investigation, in this view of the necessary state of things; and must hope that it will excite others to examine into matters of much more importance than those in which they now seem engaged. It must be plain, at least, that the theories which, in our systems, represent all the strata enumerated after the coal series, as following it in an order of superposition, are erroneous; while we have not at present any proof of such superposition, beyond the cases in which it is actually found, or can be fairly inferred by the usual rules applied to the investigation of consecutive strata.

To enquire of the revolutions to which the coal series may have been subjected, it is necessary to proceed to the next stage in the changes of the earth's surface, or to that set of strata which immediately follows it. Did none follow, and did it now remain on the surface, in a state of nudity as of repose, we might imagine that the earth had undergone no more revolutions. In some places, it is actually the uppermost, as is the case in Scotland, But, even there, it is not horizontal and undisturbed, as it ought on this supposition to be: whence we may safely conclude, that all the coal strata yet known have undergone an analogous if not a simultaneous disturbance, whether now actually followed, or not, by those which, in some places, succeed to them.

It might be sufficient here to name the magnesian limestone and the red marl as two strata immediately following the coal with us. Where that series is not present, they succeed, of course, the next inferior stratum, as might be expected. Now these are of marine origin; since they contain marine remains, as do the multifarious beds, up to the Chalk, which follow them. Their materials were therefore deposited under the ocean, before being indurated and elevated as we now find them. And when their actual superposition to the coal strata is proved, especially in an unconformable order, the doubts just stated are excluded.

Putting aside the fantastical theory of augmentations and diminutions in the ocean, under which, were it not in itself perfectly untenable, the coal series must have been horizontal, as the superior ones must have also followed in consecutive order, the land must have subsided to have produced these effects. Else, the red marl and the subsequent marine strata could not have been generated. Here then is a revolution of the surface, apparently the reverse of all the preceding; consisting in a depression of the land, instead of its elevation. Thus depressed, the ocean which covered the coal strata and all those which sank with it, became the repository of the materials for the next consecutive series of the secondary strata; commencing, as we suppose, with the magnesian limestone, and terminating at a point which does not appear to have been the same in every part of the world. In England, it is the chalk.

The effects of this change, viewed as a revolution, must have been no less extraordinary than its character; since it implies a corresponding depression, not only of the secondary strata beneath, but of the primary rocks also, with a change in the disposition of the earth, wherever it occurred, extending to great depths and of enormous magnitude. Such views as this may be alarming to minds which fear to move one step out of the mere path of observation; yet every part of the theory of the earth, thus far, is so mutually linked, and every fact so confirms every other, that we cannot stop in the chain of consequences. They who will not reason may renounce the whole, but they cannot except a part.

With respect to the probable extent of this revolution, the decision must rest on that of the Coal strata throughout the globe, and the accuracy with which they have been ascertained. The extent is sufficiently considerable to prove a very general revolution of this nature: but until it is ascertained that all these deposits correspond in geological position to those under review, we must remain uncertain. Many appear, as some are proved to be, of this nature: and even these may suffice to establish a wide revolution of this kind, while future observations will probably extend this certainty. But till those upper deposits of coal which I have here called Lignites, are distinguished every where, we must not be hasty in deciding on the extent of this particular revolution.

But the extent of the revolution by which that surface, at least, on which the coal strata were formed, was depressed, rests far less on them than it does on the next subsequent general formation, or on the red marl. Wherever this occurs, the strata on which it lies were beneath the ocean at the time of its formation, and probably at one period, though this cannot be absolutely proved. Now it appears to be as general a rock as any that is known; being assuredly far more widely diffused than any other secondary stratum, and, if the term universal is ever admissible, peculiarly deserving of it.

Hence therefore we may conclude, that not only was the earth on which the coal strata were formed, depressed beneath the ocean wherever these are found, whether covered by the red marl or not, but probably wherever that stratum occurs in any state of sequence to the inferior rocks; unless indeed a similar rock should have been forming beneath the ocean at the same time as the coal; and this being the peculiar difficulty which we cannot yet solve.

This revolution, which depressed the coal strata, will, whatever its extent may be, appear an exception to all others, in the eyes of geologists, who have overlooked this plain inference, and who also have for ever coupled the notion of elevation alone with that of revolution. But I shall soon show that the same effect has probably happened in every other revolution; though the peculiarity of this deposit allows us to prove in this case what we cannot do in the others, as it will also be an argument of analogy respecting those.

It must still be observed, that the depression of the land, even if generally granted to these arguments, could not have been universal. The red marl itself, together with many of the subsequent strata, is formed of the ruins of rocks which must have remained above the waters; lands subject to the action of the elements, giving passage to rivers, and thus contributing to lay the foundation of another world. But if the existence of dry land during this period, were not thus esteemed to be proved, the remains found in the lias would put this beyond doubt. That series is the next which was formed beneath the same ocean, and it contains terrestrial organic fossils sufficient to establish this supposition.

But, under general views, it is not easy to see how this could be otherwise; since it is to conceive a globe, of which the entire superficies was water, and thus, a kind of equalization of the solid surface beneath it. Whence the probability is, that such a revolution consisted in elevation and depression at the same time; or that while land sunk beneath the ocean in one place, its bottom was raised into land in another. And in this case it is plain, that there must have been a second disclosure, in some places, of the primary and the inferior secondary strata; producing, thus, a complication of effects as to that which I have hitherto spoken of but as a simple depression, respecting which it is easier to conjecture than to decide. I cannot yet, myself, satisfactorily make out any appearances which require this explanation, or which it would explain; not having had the requisite opportunities since I examined into this subject: though I have reason to suspect it as to some former observations which I have now no opportunity of verifying. But having pointed out to Geologists the theory, or the necessity, it is probable that they will hereafter discover the facts in question; in which case the clue is also offered for the solution of many obvious difficulties respecting the positions of the lowest secondary strata.

I must yet remark on these depressions and elevations, or changes of relative level in the sea and land, that accustomed, partly through the violence of volcanic action, and partly through the influence of the term revolution, to consider such changes as violent and brief, we are probably depriving ourselves of the means of explaining many appearances among these, and perhaps other strata. It is quite conceivable, that such a change should have been extremely gradual and tedious, resembling the slow changes of level known on the coast of Italy, and apparently in our own island. In this case the great depth of the coal strata is easily explained, as I have already hinted; and thus might we also account for the alternations of marine strata so often suspected in these deposits. Thus also we might explain certain phenomena, real or supposed, belonging to the lias and its animals; because if these were deposited on its maritime boundary at certain points, a gradual subsidence might have caused them to occupy a large horizontal space.

I have thus brought down this enquiry to a state of things, in which a portion, both of the primary and of the older secondary strata formed the dry land of the earth, as they had done before the last revolution; but, in which, a large portion of these also, which had once been elevated, were again depressed beneath the waters, together with the productions which occupied their surface. This is the fourth great revolution which the earth appears to have experienced; the three former consisting, as far as the appearances show, in elevations of the bottom of the ocean, and this one in a depression of the dry land; but under the reservations just made. And I may here remark that this offers an explanation of the modes in which deluges might have been produced, in addition to the partial torrents consequent on the elevations of strata in particular places. The submersion of the dry land over any portion of the earth, is a deluge; and, thus we possess many causes, independently of the much discussed Historical one, for explaining the cases of alluvial formations.

If cosmogonists have invented systems of depression, causing an imaginary crust of the earth to sink into imaginary abysses, they have been borne out by geological facts, of which they knew not the existence, though their theories were wrong. Too much ridicule has been cast, by recent writers, on the depression of strata as an engine of geological theory; partly because the mode of explanation was wrong, and partly, because it was applied to the solution of cases which it would not solve. But if the elevation of strata is proved, so also is the depression; while if I am right in the preceding remarks, the one implies the other. And if I have succeeded in showing that this is true of the case just examined, it is not less probable, as I already hinted, that every great revolution of the earth has possessed a similar character. Thus the several disclosures of land have been attended by a conversion of land into sea: or, in a geographical sense, the results have been interchanges, to a certain degree, between the places of sea and land. But I leave this to the consideration of geologists.

It will not affect these views of this interval of revolution, to know that in the Alps, or in any other place, the magnesian limestone and red marl are highly inclined or even parallel to the inferior strata. All that follows is, what is here often indicated, that such revolutions may not have been simultaneous nor exactly similar, nor even the same in number, in every part of the globe: and they may perhaps assist in proving what has already appeared probable from other arguments, that every period of revolution has occupied an indefinite time, and has consisted in a long succession of unequal, though similar actions.

To proceed with this investigation, I must now remark that the ocean of this last earth was the residence of all the beings now preserved in our secondary strata. If they were a separate creation, they form the last, previous to that which now inhabits the earth; as every posterior revolution appears to have been of too partial a nature to produce any change adequate to the entire destruction of life. But as the repose of this ocean, or the time which intervened between that depression of the strata which has thus been examined, and the subsequent elevation, next to come under review, is measured by the thickness of the last marine deposit, we may try to conjecture, within general and comparative limits, what its duration was. The red marl and its preceding limestone, with the lias, and the oolithe, form the most general and most widely extended portions of this deposit, and are of great depth; and if, to these, we add the remaining strata up to the Chalk inclusive, we shall have the extreme measure of repose which is attainable. Comparatively therefore, we are enabled to determine, that the period of repose during which the coal strata remained beneath the ocean, was greater than that during which they were forming on the surface of the dry land, and greater also than that during which the inferior secondary strata were generated.

Although, for the purpose of examining the revolutions of the coal strata, and the state of that ocean which followed their depression, it has been necessary to speak of the superior secondary strata as they are actually visible, it remains to mention that revolution by which they were brought to their present places. Before this event, we must still suppose that the dry land consisted of the primary and of a portion of the inferior secondary strata only; as it had done before the elevation of that portion of the secondary strata on which the terrestrial, or coal strata, were deposited. By this great, and, to us, most important catastrophe, the whole face of the earth must have been completely changed; far more, we may presume, than by any preceding one.

It is evident that it must have involved the whole of the strata, of whatever class, from the earliest to the latest of the globe. As the latest portion of the secondary strata, then reposing in a horizontal position beneath the ocean, were raised to the surface, and inclined in the manner that we now find them, so every inferior series in order, however previously inclined or disturbed by former revolutions, must have again been moved. Thus, even the primary strata, which had been twice elevated, and, in some places at least, once depressed, possibly oftener, must have been a third time raised, together with all those of successive ages which they bore on their surface. Thus also the inferior secondary strata were a second time brought up to the light of day; and thus the coal deposits were restored to those regions in which they had been originally formed. However subsequently modified by more recent partial revolutions, and by those actions of waste and deposition which are now passing before our eyes, it was by this important revolution that the present distribution of the surface was determined, in the general outline of our continents and islands, the chains of mountains which give rise to our rivers, and the cavities which received our lakes.

That it was universal as it was deep, scarcely admits a question. The wide extent occupied by the superior secondary strata, indicate the regions to which it reached; nor can we conceive it to have been thus widely diffused, without involving the whole earth. If there be a revolution, which, more than any preceding one, can be considered universal, it is this. But we have no right to suppose that it was a sudden catastrophe, or that the elevation of all the parts of the present dry land was simultaneous, or even that this change occupied a short period. It is more likely to have consisted in a long series of similar events, and to have occupied ages: we are ever misled by the sound of the terms catastrophe and revolution.

On Revolutions succeeding those of the secondary Strata.

It remains to examine the revolutions of a partial nature which have taken place at different points of the surface, produced by that last and general one which I have thus described. The same general laws apply to these; they consist in elevations, and perhaps of depressions also: as we must refer them to causes of a corresponding nature, operating partially.

Wherever the rocky surface of the earth remains in that state of repose which succeeded to the last elevation of the strata, we find the secondary of the last order following each other, even to the uppermost, in a consecutive manner, and at angles of elevation which are commonly moderate, though variable. Wherever an interruption of that order takes place, we have reason to suppose that some partial cause has operated in that spot. But, besides this, we find, in certain places, the materials which form the bottom of the existing ocean, elevated to levels at which its waters could not have stood, except under a diminution of those which is not admissible from any laws of mechanics or chemistry, known or supposeable: and thence again it is inferred that the surface must have been elevated in those places, while, in certain cases, we discover the elevating causes themselves, and have even witnessed the fact.

In the 15th, 16th, and 17th Chapters, I have explained this subject: but having done this for the first time, and being thus compelled therefore to associate many different facts, that I might disentangle the confusion made by former geologists, I must here state the cases which belong to the present view. They are, first, those in which we find the bottom of the ocean thus elevated, whether we can prove a partial disturbance of the inferior strata or not, and where, at the same time, we do not discover the elevating causes; and, next, those where we perceive the same fact accompanied by its causes. This is the twofold division demanded under the present views; because each is a partial revolution, or set of revolutions, which we can apparently separate in time: the case of Italy being an example in one, and that of the Coral islands in the other.

But, under this enquiry, there are two desiderata yet unfulfilled: the observations of Geologists being hitherto worthless as to both; because they had neither seen the causes nor understood the facts: having even entirely overlooked the latter, except in two or three instances, and misapprehended them when seen. Under the arrangement and explanations which I have here made, it must be hoped that they will hereafter supply this defect, and thus enable a future systematical writer to fill up the sketch which I must leave imperfect. These desiderata are, the periods of time at which such partial revolutions took place, and the places where they have occurred. As to the former, we have seen them happen in our own day;

and we are also sure, in such cases as that of Italy and the Isle of Wight, that they are of a very remote date: but we can neither fix the former point nor conjecture what intermediate ones there may have been. Respecting the latter, we trace them in different parts of Europe, apparently of the higher dates; and over all the Pacific ocean, or more, of many different ones: some of which probably equal those in antiquity, as we perceive that others are modern, and some quite recent. And this is all that can yet be determined: while the only safe conclusion therefore, belonging to this view, is the following. It is probable that these partial revolutions commenced immediately after the disclosure of the strata under the general revolution last described, that they occurred in many different places, and that they have been continuing to occur to this day, in different places at different times: all the appearances being essentially the same, as the causes also are; while the differences are mere differences of casualty, depending on the nature, places, and extent of the strata thus elevated or disturbed. And lastly, it is further probable, that these elevations or partial revolutions were mere continuations of those actions which disclosed the secondary strata: whence we are entitled to expect that the most remote will be the most numerous and extensive, as also that these characters will determine, or aid in determining, the question of antiquity as to those partial revolutions.

At this point the present investigation stops: what remains to be said on the other revolutions of the present earth, will find its place in examining the alterations which a progressive state of waste is now causing on the surface.

General Remarks on the past Conditions of the Earth.

As the evidences and details necessary for the preceding views have unavoidably given a still more intricate appearance to that which is an intricate subject in itself, it will be convenient to the reader if I commence these remarks with a brief summary of the conditions of the earth which I have attempted to establish.

The first condition which I have inferred, was that which furnished, from its rocks, the clay, sand, and mica of the fragments imbedded in our primary strata, because these belong to a preceding set of strata. Of this Earth we can have no other visible evidence, as there can be none at all for an Earth before it: and I have therefore taken it as the first, though a prior one might be supposed, on the obvious supposition that these materials must have arisen from rocks still anterior, under similar circumstances.

The first revolution also of which we have any evidence, is that which elevated the materials contained by the first ocean to form the rocks of a second world.

Of this second world, we see the fragments of the strata, preserved in the conglomerates of the present primary rocks, together with fragments of the granite by which these were probably elevated. It is not unlikely, that the very strata themselves form part of our primary ones, since this can scarcely fail to be the case, for the obvious reason that they could not have all been destroyed, and must have been re-elevated together with them at the subsequent revolution. The distinction ought to consist in nonconformity of order among the present primary strata, which should thus display at least two divisions. But geologists having never imagined such a state of things, having never yet reasoned in this very obvious manner, have never sought for such distinctions. I think that I can quote cases: but am averse to do it while I remain unsupported by others: hereafter, I doubt not that it will be fully established; now that the fact is indicated and the road to the investigation pointed out: in which case a subdivision of the primary strata will become necessary. A second elevation of the strata raised these above the waters, and thus was produced the third state of the Earth.

This third world is preserved for our inspection. Those which were once its only strata, are now our primary ones; and they furnished the materials for those new rocks which appear in the fourth state of the globe, and which were raised above the ocean by a third revolution.

By that revolution was formed the fourth world; the greater part of which still remains, forming the foundation of our own, however modified by numerous subsequent changes. This world included our primary, and the earlier, or lower portions of our secondary strata, producing, during its continuance, the coal deposits; but the revolution which it underwent to a fifth state, though probably not of a different nature from the former, exhibits, to us, the marks of depression only, without any proofs of elevation. The new dry land became sea; and the lands formerly elevated, were, to a certain degree, or extent, lowered.

Thus was produced a fifth world. How far it differed from the preceding we cannot discover, because we are not certain that the last or fourth revolution was an extensive one, and do not know what might have remained, and what might have been elevated. It is possible that it may have resembled the third, that none but primary rocks formed the dry land: but it is equally possible that both the primary and secondary might have united to form it.

The sixth world is that which we inhabit. It is the produce of a fifth revolution in the globe. To form it, the depressed strata were again elevated; whatever else might have occurred. Thus it contains, together with its own new ones, the strata of the three preceding states of the earth. These are easily demonstrated; namely, the primary, the first order of the secondary, the coal strata, and the last of the secondary. It is probable that those which were formed beneath the water, during the period of the formation of the coal strata above it, are also known; but they have not been demonstrated as separate and defineable strata. And these are the great or general revolutions of the Earth, as far as we can discover them, first, and as far as we can discover what is general as distinguished from partial. Those which follow are certainly partial, though of various degrees: and they have probably occurred in different places, at different times, commencing with the last general revolution, and extending down to our own days.

Whether the original globe was inhabited or not, we must ever remain ignorant; but I have shown that during at least five conditions before the present, it consisted, as it does now, of rocks, or of land elevated above the water, and of an ocean. The foundations of each successive new series of rocks on the earth's surface, were laid beneath the preceding ocean, except in one instance; and they were the produce of materials washed down from the land into their submarine receptacles: these actions being analogous to those now going on, as the submarine alluvia of our earth are destined, at some future period, to form new rocks, and to generate a new earth. By the elevation, in succession, of these submarine deposits, they were brought above the surface of the waters, forming, in each, the dry land of that terraqueous globe which was again to undergo similar and succeeding changes. In one case alone, the only assignable change was a depression of the dry land. Thus the original earth, original at least to us, is traced, in a general manner, to that which we now inhabit.

We have reason to think, that under each of these successive states, the earth must have possessed different characters, and, that its geographical distribution was different in each; but we have no reason to believe that its rocks presented any other varieties than those which we now see. The primary strata resemble those which immediately preceded them, as is proved by the nature of the included fragments; if we cannot carry this comparison further backwards. The greatest change of character takes place between the primary and the secondary, but the analogies continue; while I have formerly accounted for the differences that do exist. That every former state of the earth contained, like the present, alluvial soils, is certain, because that is implied in the formation of submarine alluvia.

Similar conditions as to an atmosphere and all the consequences which it implies as connected with an earth, also follow: and, that the lakes, of one state at least, were of great size and number, is proved by the formation of the coal deposits. But if the sea and the dry land were differently distributed, there is no reason to think that the absolute quantity of water has varied at different periods, since there are no phenomena that render it necessary or probable; as there are many that render it improbable or impossible. A larger portion of water needs not be assumed for the solution of the materials of rocks, because they are insoluble in any quantity that could have existed consistently with the order of the planetary system; as the existence of such an imaginary quantity is chemically improbable, because we know of no means by which it could have been converted into other substances, and because nothing is annihilated. Nor would the alternate elevation and depression of water, even could it be admitted, account for the geological appearances of the successive orders of strata; while their inconsecutive positions, with other facts, prove that the land was elevated. Nor is there any reason to think that the quality of the ocean underwent any changes. That it was a salt ocean, at least when the red marl was deposited, we are sure, from the quantity of salt in that stratum; however difficult it may be to account for the mode in which it there occurs: and that it was a salt ocean in prior states of the earth, we must equally presume, from this and from the early organic fossils.

How far the temperature has been constant, and similar to what it is now, I formerly enquired; and need only remark here, that the existence of marine animals in former oceans, proves that there never was any steady or considerable difference between former temperatures and the present one, during the periods of an inhabited earth's repose. But if the elevation of strata was produced by the protrusion of fluid rocks from beneath, it is probable that the Earth did actually experience temporary and considerable vacillations of heat; causing, in particular, important changes in the water and the atmosphere. In such cases, supposing such a revolution universal, we must presume that all life was extinguished, to be renovated at a future period; and that this period could not have arrived till the temperature of the surface had again subsided. Under this supposition, we have also an atmosphere, to be reduced to its present condition by the precipitation of its water, and the consequent formation or increase of seas and lakes. In the last of these revolutions geologists may therefore seek the developement of Chaos, so often discussed; together with what is more important, hydrostatic actions capable of producing many of the phenomena which have been sought in deluges of more recent date.

Respecting the inhabitants of past conditions of the earth, I have shown that the first which can be traced are those which existed, as animals, in the ocean of the second condition here enumerated, being that under which the primary strata were formed, and, probably, as vegetables, on the land of that second earth. There are also reasons to conclude that there has been a progressive increase of the numbers of animals in successive states of the globe, or, that the number in any one condition was greater than that in the preceding; as the present population seems, similarly, more abundant than that immediately prior. If that can not be satisfactorily proved by the absolute presence of the organic fossils themselves, it is safely inferred from the gradual and successive increase of calcareous strata: and thus does the presence of coal, including the later, or the lignites, with the great coal deposits, indicate the existence of a vegetable surface in the earth, from its fourth condition onwards; though there is, in this case, no proof of a regular increase in the numbers or quantity of plants, analogous to what we conclude of animals from the constant increase of the calcareous strata.

Thus the state of the earth has been meliorated by some, if not by all of its changes; or it has been rendered successively more fit for the habitation of succeeding races of animals. The last improvement which it has undergone, in the newest calcareous strata, so much exceeding former ones, is palpable; as every fresh change appears to add new races, or at least to increase numbers, while the revolutions which follow, though they should destroy their forms with their lives, or destroy those preserved from antecedent worlds, leave the calcareous earth into which they are resolved, unchanged. In their present state, the softer spoils of animals form a manure for that land which supports the existing races of animal life by its vegetable produce. Their harder parts, accumulated through the various revolutions of the globe, tend equally to increase the most valuable soils, by forming the calcareous tracts so noted for their fertility. The manure of ages past, they become that of ages to come, since they are inexhaustible: and as they are also perpetually increasing, we may imagine that a future world, when one shall arise from the ruins of the present, will as far excel it as this on which we live is superior to that which preceded. This can never concern us: yet in contemplating the revolutions by which Life has so often been widely destroyed, it is pleasing to reflect that every change is improvement, and that, out of evil, good is for ever produced. And thus seeing that the perpetual progress of the earth is towards improvement, while that progression leads us to imperfection as we trace it in the reverse order, we also perceive the evidences of that continued Design and Superintendence which philosophy has so often been accused of disbelieving, as geology now furnishes me with those demonstrations of them which have never yet been derived from any other department of nature, and which have been hitherto overlooked by that science itself.

On the Causes of the Revolutions of the Globe.

The general causes of these revolutions have been suggested in the former remarks on the elevations of the strata and on volcanoes. We perceive, in the first place, that volcanoes eject fluid matter which becomes rock, and that the force which ejects this fluid, be its nature as obscure as it may, is one of great power, since it is capable of elevating the superincumbent strata, even where these consist of such enormous masses as those which form the coral islands. And though we have not ourselves witnessed the elevation of such an island, we can entertain no doubt of the fact, because existing men, and men under the records of history, have witnessed similar ones; as the nature of these islands is also perfectly explained in this manner, and can be explained in no other. To have seen Owhyhee brought up from beneath the ocean, could scarcely afford more perfect conviction.

Again, we find that the rocks produced by these fluids resemble others which are disposed all over the earth, consisting in the trap rocks, including all porphyries, and the granites; the volcanic rocks being gradually undistinguishable from the traps and porphyries, and the latter as gradually passing into the granites. In the next place, the collateral appearances attending the trap rocks resemble so exactly those of the volcanic ones, that geologists have never yet been able to draw a line between them, either on this point or in their mineral characters: while, in the same manner, those which attend the granites resemble those belonging to the traps, precisely. And lastly, what is here the most important fact, the connexions of the superincumbent and neighbouring strata with the traps and the granites, are exactly those which subsist between the former and the volcanic rocks; consisting in the elevation, displacement and fracture of all the strata with which they interfere.

In every point therefore, the cases are identical; and thence I conclude without fear, that wherever any of these rocks occur, or can be inferred, there have existed volcanic actions, if not eruptions, and that these have been the causes of the elevations of the strata, or of the revolutions of the earth; which consist in such elevations, or in elevations balanced by depressions; that is, by a vacillating or interchanged condition of the surface of the solid sphere as that relates to the surface of its fluid portion. And the general conclusion consequently is, that the Earth has, from any beginning which we can trace, been the seat of a succession of similar actions, produced at different intervals; and those intervals, when conspicuous or extensive in their effects, causing the revolutions of condition here described.

If there are differences in any respect between the natures of these several rocks or the collateral appearances attending them, the causes have been formerly explained, nor do they affect this reasoning. But I may here refer to the Moon in illustration of this condition of the globe of our Earth; while all analogy makes us infer that every planetary body is of the same essential nature. It is a congeries of mountains, of which the far larger portion is visibly volcanic, as all may have been; while we trace, with perfect security, successions of volcanic mountains, or of eruptions, in which every part of its surface has been engaged at different times, as its volcanoes still occasionally burn. And there we also perceive the probable reasons why our own surface differs from that of this planet; especially, why the Earth has lost so widely those decided proofs of former volcanoes which that has preserved in the integrity of its innumerable Craters. It contains no water, and possesses a very slender and limited atmosphere: whence it has not been subjected to the same causes of waste which are ever acting on the surface of the Earth. But thence also, its volcanic or unstratified rocks have never been obscured by the strata which have covered those on our own surface; whatever revolutions analogous to those of the Earth it may have undergone. The volcanic actions in the earth have often elevated the strata without allowing the fused rocks to break through them, as happens daily at present; and the more delicate volcanic records have also disappeared in the successions of waste and of revolutions, as must be especially plain of all the earlier ones.

The perfect simplicity and consistency of this view, with the clearness of the proofs, leave nothing to wish as to the theory of the revolutions of the earth; and I know not that I ought to answer a trifling and ignorant objection often made, namely, that these intervals of revolution, or action, succeeded by dormancy, were the convenient assumptions of an hypothesis. It has been shown that the actions have probably never ceased, as they are still going on: they have been augmented at intervals: but under final causes so demonstrated in the present chapter, that to deny the facts, is to deny the existence of a Power regulating as it created the Earth; by whatever means the secondary causes are managed to those ends. And while the existence of a perpetual source of heat and of a fluid condition within the earth, is proved, while it is also proved that the force of this heated fluid is constantly producing the very effects in question, and also producing them at intervals, thus uniting, now, every thing necessary for the former effects, I cannot see how such a theory can be called gratuitous, or in what manner philosophy can ever prove any thing, if it has not thus proved a permanent cause, adequate to every end, under every conceivable variation. The demonstrations of mathematics can compare with nothing; for they are but the catenations of successive syllogisms, from assumed, or established, simple facts or abstractions: but physical science never yet has offered, nor ever will produce, proofs more perfect than Geology has thus done respecting the causes of the Revolutions of the Earth.

It remains to enquire what actual evidences can be found, that every such revolution was attended by the formation of an unstratified rock; and I think, that with little difficulty, I can produce them, even where it might seem most difficult, namely, in the earlier periods.

I have shown that fragments of granite are entangled in our primary strata, and this therefore is the very granite which elevated the Earth which I have here called the second; while, if the quartz of the other fragments be derived from granite, as is probable, it will be evidence of an anterior one which elevated that one here marked as the first; the evidence, as I formerly explained, being incapable of reaching further. It demands a second production of granite, or of the older porphyries, which are of the same general æra, to elevate the primary străta; and a third to effect the elevation of the lowest secondary ones. Now, of these, we have the most abundant evidence in those granites, of which the veins of one kind traverse the mass of another; and thus I have provided for those conditions here called the third and the fourth. Again, it requires a fourth production of an unstratified rock to produce that change which attended the depression of the coal strata. Now there have been granites found, where a vein of a third granite traversed a vein of a second and different one which had previously traversed the mass of a first; but if this should be doubted, there are two antient porphyries which successively traverse a mass of granite, the last of these being, of course, the third production.

Thus is every necessary cause of revolution demonstrated as to the fifth earth of this arrangement; while, from this point, I can no longer certainly trace any further mutual interferences of granite or antient porphyry. But that is of no moment; since there are abundant resources in the production of the trap rocks, of which two formations, or perhaps more, are demonstrated, adequate to even more than is required, while they account for the sixth and last condition of the Earth. Beyond this, the demonstrations are admitted by every one, in the volcanic rocks of higher antiquity, and in the disputed traps; and, lastly, in those of existing or recently extinct volcanoes. And thus is every thing that can be demanded in proof of the assigned causes of these revolutions, demonstrated by the clearest evidence. The theory is perfect, even to its details and to those of each individual action.

I must not however quit this subject, without noticing a theory which supposes abysses within the earth, and subsidences of the strata into these; especially as I have pointed out what the fabricators of this hypothesis never had shown, the demonstrated subsidence of portions of the surface, in antient times, as similar phenomena now occur, in a partial manner, in volcanic countries. With them, it was pure hypothesis without a fact in support. The appearances proving elevation are so universal and decisive, that this needs not be insisted on. There could not be an abyss in a fluid mass; and if the earth be therefore such, whatever cavities it may contain must be in its solid parts, and thence, comparatively superficial; as its high specific gravity also shows that such cavities cannot be extensive, should they exist; and as we cannot even easily conceive a cavity under the actual or neighbouring presence of a fluid. And I have also shown how these depressions might take place through a mere change of relative level, without the necessity of any such supposition. Yet there are indications of a peculiar structure in the earth which might be called cavernous, if we allow that these are filled by a fluid; as they are otherwise not conceivable. These are, the linear directions of volcanoes, and the singularly linear tendencies of the elevated edges of strata. On the nature of this disposition it seems to me, however, in vain to speculate; and I willingly therefore leave it to the researches and reasonings of future geologists. I can conceive it to have been produced by former displacements of solid strata: but that is all.

General Conclusions.

In reviewing what has past, we cannot help being struck with the magnitude of the forces which elevated the strata, with the enormous power for example which, for the last time, brought up from the bottom of the ocean our present primary mountains together with their successive superstructures; the entire mass and load of all the rocks, from the lowest sandstone to the chalk. The imagination is lost in reflecting on such forces, as it is on the power which projected the planetary bodies in their orbits. Yet this force is proved by the facts: and though it were not, why should it be doubted? That Hand which spanned the globe of the earth and launched it into space, might surely move its parts. The unperceived influence of an antient atheistical philosophy is for ever shutting our eyes to the First Cause: we dwell on secondary causes till we forget that they are but the agents of Him who appointed and governs them, and who governs them for His own ends.

He who created, does He not also regulate? Or where is the difference? We are misled by a term: we are thinking of the metaphysical question of the creation of matter, when we imagine that we are discussing that of the earth. The Deity created more than mere matter, or a simple globe; but he did not create those forms and dispositions in an instant. The Earth is not yet created: nor will it be, as long as it has a change to undergo. What He did at first He is doing daily: let His power in creation be granted, and we grant His powers in regulating, in governing: it is enough.

The extent of these forces has been that of the whole Earth; but their depth must also have been enormous. Mathematics teach us how to calculate, from the extent of action, the depth of a military mine.

If we cannot actually apply the rule in such a case, we can at least conjecture, within certain limits, what was the depth of such a force as elevated any single mountain chain, of which the continuous direction and uniformity of position indicate a single uniform action, as does probably the earthquake of Lisbon. Could we suppose that Europe had been produced by one effort, the focus of the paraboloid would be scarcely less distant than the centre of a solid earth. But the reader can reflect for himself; the data are before him.

Nor must the great question of Time be passed over lightly. We are too apt to measure this by our own brief duration, as our vanity dreams that the universe was created for us. Let us contemplate Time as it relates to the Creator, not to ourselves, and we shall no longer be alarmed at that which the history of the Earth demands. Every change which it has undergone has required time: every new deposition of rock has been the work of ages; and the sum of these is the duration which has been reviewed; although this is possibly but a small space compared to that through which it has existed as a planetary globe. Every stratum of rock is the work of time, often of far more than we choose to contemplate: while from what we see, we can approximate to that which we know not how to measure. He who can measure and number the strata from the first to the last, is prepared to solve this question as it relates to the intervals of repose, but of those only, not to those of the revolutions: let him ascertain the time required to produce a stratum of a given depth, let him seek it in the increase of colonies of shell fishes, in deposits of peat, and in the earthy deposits of seas and lakes, and he has found a multiplier, not to disclose the truth, but to aid his imagination.

Who indeed can sum this series? the data are not in our power: yet we can aid conjectures. The great tract of peat near Stirling has demanded two thousand years; for its registry is preserved by the Roman works below it. It is but a single bed of coal: shall we multiply it by a hundred? we shall not exceed, far from it, did we allow two hundred thousand years for the production of the coal series of Newcastle, with all its rocky strata. A Scottish lake does not shoal at the rate of half a foot in a century; and that country presents a vertical depth of far more than three thousand feet, in the single series of the oldest sandstone. No sound geologist will accuse a computer of exceeding, if he allows six hundred thousand years for the production of this series alone. And yet, what are the coal deposits, and what the oldest sandstone, compared to the entire mass of the strata? Let the computer measure the Apennine and the Jura, let him, if he can trust Pallas, measure the successive strata, of sixty miles in depth, which he believes himself to have ascertained, and then he may renew his computations; while, when he has summed the whole, his labour is not terminated. But let the reader supply the figures which it is useless to exhibit, since they cannot be true.

If these views of the powers and the results of geological investigation are alarming to feeble minds, they tend to exalt that science in the estimation of those who neither fear to seek Truth, nor dread it when found. It is not astronomical science alone which will hereafter elevate the mind in the contemplation of the universe: but the Earth has hitherto been reasoned on by mathematicians only, and all but its mechanical relations have been forgotten. If there are still ignorant and anxions persons who think that these views interfere with our Faith, I refer them to the following chapter; for I will not agitate this question twice. The learned reader will not fail to remark their coincidence with the opinions referred to the Stoics, respecting the destruction and renovation of the Earth at fixed periods. But it is easy to trace these doctrines, and the Great year of Aristotle, to Pythagoras, and, beyond him, to the far more antient philosophy whence Greece borrowed more than its astronomy and mythology. If it was a Chaldean doctrine, so was it that of antient India. The Calpas of the Brahminical school are the periods in question: and hence the "magnus ab integro sæclorum nascitur ordo," of those who copied more than they invented. But if there be any one who imagines that these opinions were the result of observation, and not the speculations of hazard, that the East had made those advances in Geology which it had in Astronomy, he may please himself by dwelling on that singular coincidence through which we are now in the fifth Calpa.

If I have attempted to trace backwards to that furthest state of our globe respecting which we can procure any evidence, yet not thus tracing the first member in this series, nor thus excluding many former vicissitudes and a long preceding existence, I have also, in tracing a progressive melioration, retraced to points in succession where organic beings were less various and abundant, and especially where animals continue to diminish, from the least questionable of evidences, that of the gradual diminution of the calcareous strata. In such a diminishing series, the first point is unity; and, beyond it, is nothing. Could we therefore fix this point, we might discover when the Creation of animals commenced. We have not done this: yet so far are we from being called on to believe that the creation of animated beings has no retreating limit, that we must conclude it to have had a commencement, and one also, the date of which we can assign in a general manner, as compared to the æras of the earth itself.

I read, that as the variations which astronomers have discovered are periodical, and that as the solar system presents no marks of a termination, so it discovers no indications of a commencement. I read also that the phenomena of the earth coincide with those of the Celestial mechanism, and further, that the laws of animated conform to those of inanimate existence. I read in the first assertion a partial truth, whence the mathematician has deduced what is not deducible, forgetting the very rules of his own science; and in the two latter assertions, I read what are not truths. If I am surprised at the more than oversight in the whole of these assertions, I will not note the purpose, since it is evident. But I must answer the mathematician: for this is my duty. The answer cannot indeed now reach him: but it will reach those whom he has misled.

That a machine may perform an appointed duty, it must be a finished work: it must be perfect from its commencement; and while it does continue to perform that duty, it remains perfect. The celestial mechanism was designed by its Creator for a final cause, an End; and that end is still in view. Unless therefore we doubt His Power or His Wisdom, that mechanism could be no other than it has been and is. The order of the planetary system on which the mathematician dwells, is its necessary condition: but no sound logician will infer from this that its past duration has been eternal, or that its future has no bounds. It must have been precisely what it is, though its duties were temporary; and it may therefore be temporary, as well as eternal, for all that its regularity proves. Of its future limitation we can infer nothing: except that this is within the power of Him who designed and executed it: of its duration in the past, I shall presently show reasons for believing that we can prove a commencement, though not the date of that.

When I say that its duration is within the power of its Creator, I do not allude to a mere act of Will. The secondary causes are ready, and ever active. The Mathematician, accustomed to the sole contemplation of his own science, has forgotten that the laws of Mechanics comprise but one of two great powers in the Universe. Chemistry is the other right hand of the Creator: the source of change, the joint governor with Mechanics; the opposing power, when its power is required. This mathematician, writing on Geology, should not have forgotten that: as a mere astronomer, he ought not; for that Chemistry is acting in the Comets and in the Sun, as it has acted and is acting in every planetary and solar body throughout the universe. Its great agencies have been overlooked or forgotten in the exclusive pursuits of the Mechanical philosophy.

It is said also that the laws of animated coincide with those of inanimate existence. I have answered this already. I have shown the high probability that Life has been more than once destroyed and renovated, that it has had many commencements; the certainty, assuredly, that it must have had at least one, and that one at a far later date than the creation of the Earth itself; though this is implied in the very production of the Earth within Time. How that point is related to the commencement of the earth itself, it is in vain to speculate. It is a moral argument for their near coincidence, that the object of the Deity in Creation was Life: yet to Him to whom Time is nothing, it were nothing though an unanimated globe had revolved for ages in the general system. And any second commencement of Life implies a previous destruction; whence it is not necessarily perdurable in the future, as the causes of its destruction are also provided, in the great Chemistry of the Earth. The assertion is therefore what I called it; and this imaginary analogy is consequently nothing.

When, lastly, it is said, or insinuated, that there are no marks of a commencement in the Earth, the answer is also found in what has preceded. The retrocession of forms which I have traced, leads us constantly back to a preceding condition, under which the form of the surface was simpler, and the rocks less numerous than in the next succeeding one: under which retrogradation, we exhaust, especially, or approach to the exhaustion of, the calcareous strata. The last earth in this backward chain, though I should not have discovered it, consisted but of a single series of strata; and a preceding one should therefore have possessed none. Whatever rocks it did contain, must therefore have been the produce of fusion; and from these were, of course, formed the first strata. This inference is inevitable, from what has preceded; and therefore, though every visible unstratified rock is posterior to the strata with which it is connected, there is, or has been, an unstratified rock older than any strata. This is the first formation of rock; and it is the produce of cooling. Hence the state anterior to that, place it where we may, is the fluid state which so many other phenomena tend to prove; and thus are we carried back, by a connected series of facts and inferences, to the proof of that which was probable on other grounds; to an originally fluid globe. And if this is not the commencement of the earth, or its creation, it is still that approximation which implies a commencement; while if we admit La Place's views, and believe that this fluid was the produce of a previous gaseous sphere, the approach to creation becomes still nearer, though, at this point, we must stop. But stop wherever we may, the facts still prove, that the phenomena of the Earth do not coincide, as is said, with those of the Celestial mechanism, that the Earth was not the same at previous periods as it is now, and and that it also bears, as I have formerly shown, the marks of a progressive and steady melioration.

Therefore was it created in time; and therefore does it not support, by the asserted analogy, an equally imagined Eternity of the Celestial system, or of the merely Solar one. Let a peculiar philosophy fancy what it may, it receives no assistance from facts: on the contrary, these contradict it. The Peripatetic philosopher must submit: for even on that which had no foundation, the bolt has fallen.

But Geology can effect yet more; and with that I shall conclude. There is nothing in the celestial mechanism to prove that the Solar system, the Universe tself, is of a more remote date than Man. Astronomy furnishes no evidence. But Geology teaches us that a terrestrial globe had a long prior existence; and thence does it prove the equally remote antiquity of the Solar system, though mathematics cannot as yet so far demonstrate its connexion with other systems, as to prove that the date of the creation of those is involved in our own, as that of the Earth is with the entire mechanism of the Solar group.

Of such high value is geological science, when duly viewed. As far as what has now passed involves a Theory, it consists of those deductions from facts which philosophy is justified in drawing, but it is not yet a Theory of the Earth.

END OF THE FIRST VOLUME.

J. Teuten, Printer, 14, Dean-street, Soho.

14 DAY USE
RETURN TO DESK FROM WHICH BORROWED

EARTH SCIENCES LIBRARY

This book is due on the last date stamped below, or
on the date to which renewed.
Renewed books are subject to immediate recall.

~~SEP 2 3 1976~~	
~~REC. CIR. SEP 9~~ '76	
~~JUN 1 5 1978~~	

LD21—32*m*—1,'75
(S3845L)4970

General Library
University of California
Berkeley

YB 29851

-897